DOCUMENT RESUME

ED 245 917 SE 044 651

AUTHOR	Kilbourne, Brock K., Ed.; Kilbourne, Maria T., Ed.
TITLE	The Dark Side of Science. Proceedings of the Annual Meeting of the Pacific Division, American Association for the Advancement of Science (63rd, Santa Barbara, CA, June 23, 1982). Volume 1, Part 2.
INSTITUTION	American Association for the Advancement of Science, San Francisco, CA. Pacific Div.
PUB DATE	30 Aug 83
NOTE	221p.; The symposium reported here as part of the annual proceedings was sponsored by Section K (Social, Economic, and Political Sciences).
PUB TYPE	Collected Works - Conference Proceedings (021)
EDRS PRICE	MF01/PC09 Plus Postage.
DESCRIPTORS	*Codes of Ethics; Economics; Federal Aid; Federal Regulation; Higher Education; *Legal Problems; Research Methodology; *Sciences; *Scientific Enterprise; *Scientific Research; *Social Problems
IDENTIFIERS	*Fraud

ABSTRACT

This book constitutes the proceedings of a symposium ("Science, Deviance, and Society") designed to provide a constructive response to the social problem of deviance in science (departures from the prescribed and proscribed norms of science). Individual papers are presented in five parts. These papers are (in part one) "The Games of Science: Science Watching" (A. C. Higgins) and "Frauds from 1960 ao the Present: Bad Apples or a Bad Barrel?" (William Broad); (in part two) "Science, Deviance, and Society" (Maria Kilbourne, Brock Kilbourne, and A. C. Higgins), "Another Sociological Perspective on Deviance in Science" (Jerry Gaston), and "Anomaly Revisited: Rules and Roles in Research" (Paul Meadows and Peter Meadows); (in part three) "Fraud in Science: Who Patrols and Who Controls?" (Albert Barber), "Compliance and Noncompliance with Federal Regulations for the Protection of Human Subjects" (Charles McCarthy), and "The Legal Basis for Federal Prosecution of Science Misrepresentation as 'White Coat' Crime" (Eugene Dong); (in part four) "Deviance in Science: The Depletion of a Vital National Resource" (Brock Kilbourne and Maria Kilbourne); and (in part five) "The Inversion of Mastery" (Edward Sampton), "The Mythical Content of Economic Theory" (Mike Reed), and "Normative Deviance of Modern U.S. Science: Its Ideological Legitimation and Philosophical Foundation" (Roger Dittman). A summary by Robert Forthman ("Three Perspectives on Deviance in Science") is included. (JN)

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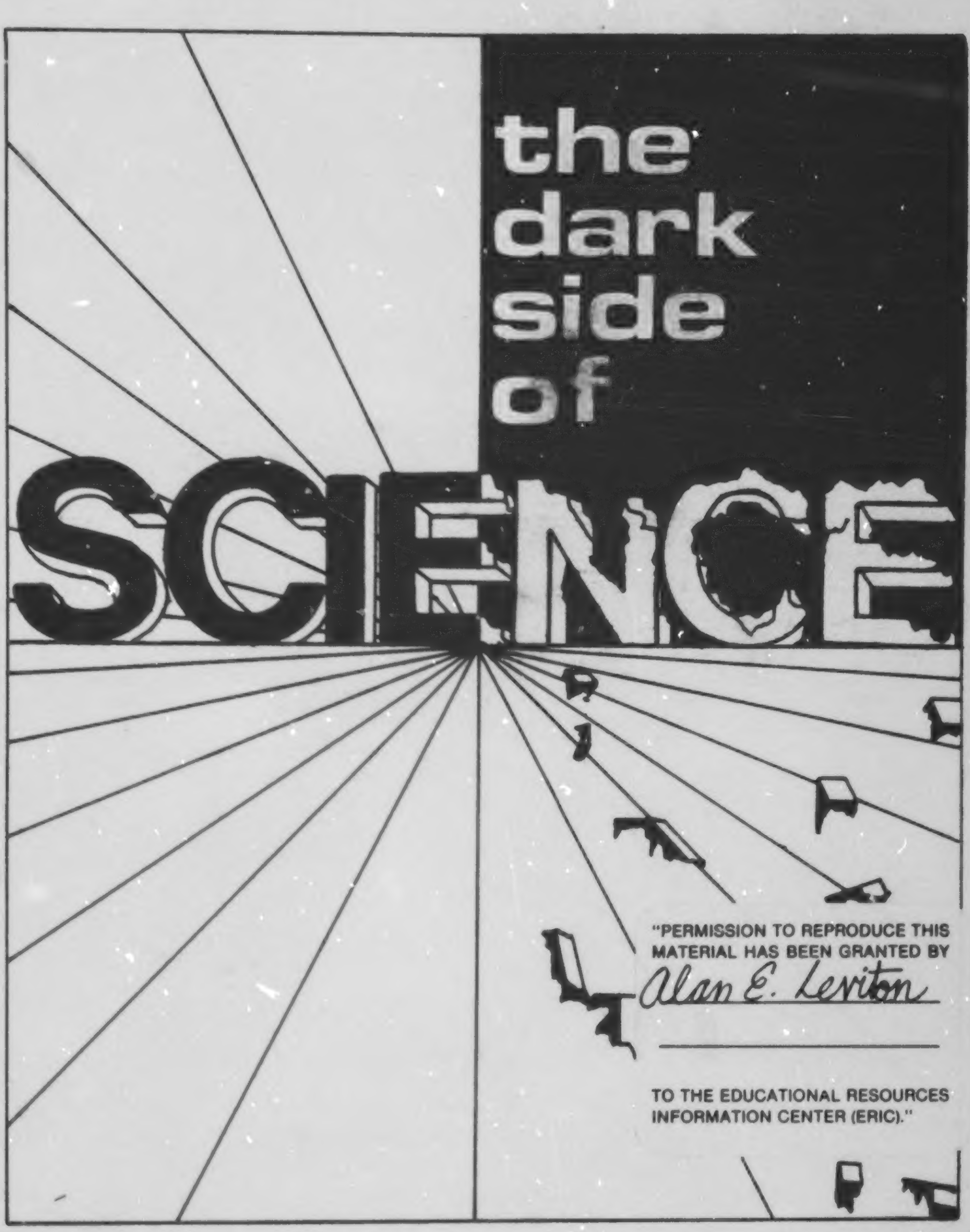
Volume 1, Part 2
August 30, 1983



edited by
Brock K. Kilbourne
and Maria T. Kilbourne

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PROCEEDINGS
of the
63rd ANNUAL MEETING
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Volume 1, Part 2 August 30, 1983

THE DARK SIDE OF SCIENCE

Edited by
Brock K. Kilbourne
and
Maria T. Kilbourne

Pacific Division
American Association for the Advancement of Science
San Francisco, California
1983

SPONSORSHIP

In June, 1982, Section K (Social, Economic and Political Sciences) of the Pacific Division, American Association for the Advancement of Science, sponsored a symposium titled *Science, Deviance, and Society.* The participants submitted manuscript versions of their presentations for publication in the *Proceedings* of the Pacific Division. Drs. Brock and Maria Kilbourne, conveners of the symposium, agreed to serve as editors for the publication.

The papers which appear in this issue of the *Proceedings* have been peer-reviewed. Otherwise, they have been only lightly edited, principally to insure consistent format. The opinions expressed by the authors are their own and do not necessarily represent the official views of the Pacific Division or the national AAAS. Neither the Division nor the program coordinators sought to restrict or otherwise direct the opinions of the contributors; their sole responsibility was that of providing a vehicle for the presentation and discussion of differing views via the medium of a symposium and the subsequent publication of the symposium proceedings.

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Golden Gate Park
San Francisco, California 94118

Manufactured in the United States of America by the Hamilton Press, Palo Alto, California 94301

TABLE OF CONTENTS

Page

Preface 5

Part One – Deviance in Science: Then and Now 7

The Games of Science: Science Watching, *by A. C. Higgins* 9

Frauds from 1960 to the Present: Bad Apples or a Bad Barrel?, *by William J. Broad* 26

Part Two – Three Sociological Perspectives on Deviance in Science 35

Science, Deviance, and Society, *by Maria T. Kilbourne, Brock K. Kilbourne, and A. C. Higgins* 37

Another Sociological Perspective on Deviance in Science, *by Jerry Gaston* 55

Anomaly Revisited: Rules and Roles in Research, *by Paul Meadows and Peter Meadows* 66

Part Three – The Administrative Response to Deviance in Science 89

Fraud in Science: Who Patrols and Who Controls?, *by Albert A. Barber* 91

Compliance and Noncompliance with Federal Regulations for the Protection of Human Subjects, *by Charles R. McCarthy* 101

The Legal Basis for Federal Prosecution of Science Misrepresentation as "White Coat" Crime, *by Eugene Dong* 108

Part Four – The Depletion of a National Resource 131

Deviance in Science: The Depletion of a Vital National Resource, *by Brock K. Kilbourne and Maria T. Kilbourne* 133

Part Five – The Larger Issue: Deviance of Science 155

The Inversion of Mastery, *by Edward E. Sampson* 157

The Mythical Content of Economic Theory, *by Mike Reed* 172

Normative Deviance of Modern U. S. Science: Its Ideological Legitimation and Philosophical Foundation, *by Roger Dittmann* 187

Summary: Three Perspectives on Deviance in Science, *by Robert C. Forthman* 217

PREFACE

This book constitutes the proceedings of the symposium, "Science, Deviance and Society," presented during the annual meeting of the Pacific Division of the American Association for the Advancement of Science, held at the University of California, Santa Barbara on June 23, 1982.[1] The symposium was organized by the editors under the auspices of the Pacific Division's Section K (Social, Economic, and Political Sciences). Members of the Executive Committee of Section K who played an instrumental role in supporting the program were: Bernard Hanes (President), Asoke Basu (Vice-President), Albert Pierce, and Ralph Segalman (both committee members).

In organizing this symposium, the editors sought a diverse range of perspectives and backgrounds of participants. The purpose of the symposium was to provide a constructive response to the social problem of deviance in science. The diversity of views presented indicates there are competing definitions of the problem. Different definitions and conceptualizations may be more or less appropriate in different situations, although the temptation to place the different views into a larger conflict perspective is compelling; that is, the definition of the problem which is most compatible with dominant interests in contemporary society may succeed, for the time being, in achieving the greatest degree of legitimacy.

Authors were given ample opportunity to revise their papers following both the symposium and subsequent peer-review of their essays. Authors accept responsibility for their own views only, and each solution or suggested solution should be interpreted only in the context of each author's definition of the problem. Similarly, no organization or affiliate of the American Association for the Advancement of Science accepts responsibility for the particular views presented by the various authors.

The articles have been organized into sections to facilitate an understanding of the issues. The sections are not necessarily exclusive of one another, for there exists overlap in certain areas. However, there is no intended correlation between the particular views of the authors and their appearance in this volume, and any imputation(s) of convergencies and divergencies of opinion is left to the judgement of the reader.

The editors would like to express special thanks to the reviewers of the symposium proceedings in alphabetical order: T. Mark Aldrich, Berch Berberoglou, Willard Day, Robert Dunn, Juan Gonzales, David Graeven, Albert Jonsen, John Nellor, James Richardson, Tom Van Cantfort, Willem Van Groenou, Lyle Warner, Robert Winzeler, and Chuck Zeh.

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[1] The article entitled "Science, Deviance and Society" is the only paper not originally presented at the symposium. An earlier version of this paper was given at the Sixty-Second Annual Meeting of the Pacific Division, AAAS, held at the University of Oregon, Eugene, Oregon, June 17, 1981.

They would also like to thank Alan Leviton, Executive Director of the Pacific Division, AAAS, and Michele L. Aldrich, Archivist, AAAS National Headquarters, for their encouragement and guidance in this effort.

PART ONE

Deviance in Science: Then and Now

Part One deals with the general discussion of departures from the prescribed and proscribed norms of science. Are fraud, the manufacturing of data, plagiarism, and other scientific skull-duggeries a dark but always with us side of science, or are such practices on the increase due to the changing research environment? In Chapter 1, Higgins argues that scientific misrepresentation is not uncommon in science and relates our concerns to the interpretations of "science watchers." Broad takes a different position in Chapter 2. Based upon his experience as a news reporter and science writer for Science, he argues that the changing structure of the research enterprise may be conducive of scientific fraud and may even tell us something about how science actually is.

THE GAMES OF SCIENCE: SCIENCE WATCHING

By
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State University of New York, Albany

In the past several years there have been recurrent reports of cheating in science, various "goings-on" associated with these names: Elias Alsabti (Broad 1980a, 1980b), Cyril Burt (Dorfman 1978; Hearnshaw 1981; Kamin 1974), John Darsee (Broad 1982a, 1982b), Robert Gillis (Science News 1977), Walter Levy (Science News 1974), John Long (Dickson 1981; Harris, et al. 1981; Wade 1981), Zoltan Lucas ("Stanford denies Coverup..." 1981) Marc Spector (Kolata 1981), Marc Straus (Broad 1981b, 1981c; Sun 1981), William Summerlin (Culliton 1974a, 1974b; Hixson 1976), Paul Todd (Broad 1980e), as well as Vijay Soman and Philip Felig (Broad 1980c, 1980d; Hunt 1981). Suspicions that dishonesty is not uncommon have been published in surveys (Ian St. James-Roberts 1976) and voiced in interviews (Gaston 1971, 1973). There have been reminders that some Big Names in the history of science may have been guilty of some of the same sorts of deceptions: Isaac Newton (Westfall 1973, 1980), Galilei Galileo (Feyerabend 1970, 1975; Koestler 1959; Koyre 1958, 1968), Claudius Ptolemy (Newton 1977), Gregor Mendel (Fisher 1936), John Dalton (Nash 1956; Thackray 1972), Louis Pasteur (Dubos 1976, esp. pp. 359-400), and Sigmund Freud (Sulloway 1979; Szasz 1979). Indeed, in the past several months some of these cases have earned comment in the press (for example, "Fraud, hoax and simple error," 1981; Golden 1981; Trafford 1981) and have been reviewed in Science with a clear call for the further study of this "dark side" of science (Broad 1981a). There have been hearings before House and Senate Committees of the Congress regarding fraud in science (Broad 1981b; Schmaus 1981; Sun 1981). All these seem to suggest that there is something of a crisis in contemporary science.

There is something particularly wrong when scientists cheat. Scientists are supposed to be dedicated, honest, trustworthy seekers after truth; science is supposed to be objective, universal, verifiable truth. In their tellings and retellings of the story of science, Western historians, philosophers, sociologists, as well as the authors of scientific texts, have typically described science and scientists in terms of Western suppositions about science. But what happens when suppositions prevail is that biographies become hagiographies and complex events get simplified. Psychological consonance requires that science be the work of great men. And it is precisely here that cheating elicits so much hand-wringing, as mere mortals are compared to idealizations.

Suspicion of science and scientists seems saner than the idolatrizing published by some. The schoolboy images of science are false and should be surrendered in an effort to understand the paradoxes of knowledge generation. History is hard enough without getting involved in myth and fable. J. D. Bernal (Bernal 1939; see also, Goldsmith & Mackay 1964) called for a science of science in the 1930s, but the idealizations of contemporary mythic science can never be a basis for such an enterprise. Rather, examining the seamy side of science may provide an opportunity to see science in a new way, a way which avoids the blinders imposed by idealized science.

American sociology of science is a poor place to start examining deviance in science. The functionalist school--identified with Professor Robert K. Merton of Columbia--specifically eschews deviance in science (Zuckerman & Merton 1971; Zuckerman 1977). That school explains departures from scientific universalism by particularistically explaining deviance in terms of temporary aberrations (Barber 1961) or situational exigencies (Storer 1977). Merton's macrosociological approach to science seems bound in and determined by its assumptions of what science ought to be.

Some European sociologists of science have employed a microsociological approach, the examination of specific cases of deviance--the Velikovsky affair (Mulkay 1969) and the operation of a single department of physics (Mulkay & Williams 1971)--as a useful alternative. These studies of specific science, in spite of some logical difficulties with methods (Mulkay 1974), have found evidences of a variety of particularisms which seem essential rather than accidental to science. Using a variation of this approach, offered here are reinterpretations of fudge and fraud, deceit and deception in science.

The Drama of Deviance

In his Autobiography, Lincoln Steffens (1958) tells the following story:

> Some of us (students and former students at Leipzig with Professor Wilhelm Wundt) were looking over the laboratory records of an American student who had stood high with the Professor and, therefore, with us all. He had gone home, taken a professorship, and was holding high our colors. He became afterward one of the leading men in American science and education. His student papers were models of neatness and as we looked we saw that they were a masterpiece of caution, wisdom and mathematical labor. The records of his experiment showed that he got, at first, results which would have given aid and comfort to the enemy and confounded one of Wundt's most axiomatic premises. He must have suffered, that promising young student; it was his thesis for the degree of Doctor of Philosophy, which he needed for his career at home; he must have thought, as a psychologist, that Wundt might have been reluctant to crown a discovery which would require the old philosopher to recast his philosophy and rewrite the published volumes of his lifework. The budding psychologist solved the ethical problem before him by deciding to alter his results, and his papers showed how he did this, by changing the figures item by item, experiment by experiment, as to make the curve of his averages come out for rather than against our school. After a few minutes of silent admiration of the mathematical feat

> performed on the papers before us, we buried sadly these remains of a great sacrifice to loyalty, to the school spirit, and to practical ethics. (pp. 124-125)

This scandal shows how fudge, fraud, deceit and deception do happen in science, just as they happen in any other sphere of work: human beings are first and foremost tied up in human, personal and social commitments and only secondarily with abstractions. Put another way, whatever commitments one might give to abstractions--truth, beauty, goodness, science--can be given only after abiding with the interpersonal commitments which are logically, emotionally and personally prior to any others. Just so, the reader can empathize with the young American's cheating: the tugs to deception were school spirit, loyalty and Der Alte Professor. These are things which are more important than straight science. To Steffens, in the specific situation in which the young American found himself, the deception is understandable. There is certainly no outrage and no shame displayed by Steffens, the masterful muckraker. Instead, the reader joins the writer in the recognition of the paradox of the human condition, where right and wrong are paradoxically intertwined.

The point here is that at least some frauds, deceits, deceptions and dishonesties are all-too-human responses to the complex situations which arise in science. These frauds are not the result of flawed personality, immortality or essential dishonesty. This is not a failure in science so much as it is a recognition of priorities: the old school tie is not a myth. One does not needlessly hurt one's mentors, one's elders. One fakes an abstraction rather than debunk a colleague. Again, to Steffens the deception is creditable.

Steffens sees such frauds as the ones perpetrated by the young American as illustrative. Steffens is not burying the evidence of this perfidy at all; here it is for all to read. The tale is a parable told for instruction. That young student is Everyman in a modern morality play.

Steffens is not trying to bring discredit to that student who, later, and perhaps because of his deceptions, became "one of the leading men in science and education." Quite the contrary, Steffens describes the student's fabrications as "a masterpiece..." Steffens implies here that this craft called science may demand more than simple obedience to the norm of universalism: the craft called science may involve a variety of particularisms which are deniable only to those who insist on self-deception in the name of idealizations. The point is, again, that at least some frauds, deceits and dishonesties are all-too-human responses to the complex situations which arise in any human endeavor, including science. Having seen that much in Steffens' story, it is possible to extend the search for other instances of humanity in science.

Other Evidences

Bell's (1937) delightful Men of Mathematics is full of gossipy stories about famous mathematicians. Here, for example, is a terse comment on Pierre-Simon DeLaplace:

> To call a spade a spade, Laplace stole outrageously, right and left, whenever he could lay his hands on anything of his contemporaries and predecessors which he could use. From Legrange, for example, he lifted the fundamental concept of the potential...; from Legendre he took whatever he needed in the way of analysis; and finally, in his masterpiece, the Mechanique Celeste, he deliberately omits references to the work of others incorporated in his own, with the intention of leaving posterity to infer that he alone created the mathematical theory of the heavens. (p. 174)

Newton's opponent in the priority dispute concerning the calculus may have been unfairly treated by Newton's biased commission, but Gottfried Wilhelm Leibniz had other things to hide. According to Bell (1937):

> Leibniz was grossly unethical over a matter--of all things--of ethics. Leibniz seems to have believed in applying his ethics to practical ends. He carried off copious extracts from Spinoza's unpublished masterpiece Ethica...a treatise on ethics developed in the manner of Euclid's geometry. When Spinoza died (1677)...Leibniz appears to have found it convenient to mislay his souvenirs of the Amsterdam visit. Scholars in this field seem to agree that Leibniz' own philosophy whenever it touches ethics was appropriated without acknowledgement from Spinoza. (pp. 126-127)

And the following quote, again from Bell, is suggestive of the reasons and rationalizations used by famous scientists in their deceptions. Concerning Frederick Gauss, Bell (1956) writes:

> Contemplating as a youth the close, unbreakable chains of synthetic proofs in which Archimedes and Newton had tamed their inspirations, Gauss resolved to follow their great example and leave after his only finished works of art, severely perfect, to which nothing could be taken away without disfiguring the whole. The work must stand forth, complete, simple and convincing, with no trace remaining of the labor by which it had been achieved. A cathedral is not a cathedral, he said, till the last scaffolding is down and out of sight. (p. 305)

So some scientists have an "edifice complex" and in the name of constructing cathedrals they can pretend that no blood, sweat and tears were involved. They can disguise their labors of discovery and obliterate the trails which led them to their "inspirations." What this means is that we do not know how, in fact, innovators got to their innovations. We do not know the process of discovery.

What else do scientists hide? In this post-Watergate, post-ABSCAM world, there are suspicions that a great deal is hidden, a great deal remains secret. Twenty years ago Leroy Wolins (1962) warned his psychology colleagues that a significant portion of then recently published journal articles could not comply with requests for their original data. These authors had conveniently "lost" their data and there was no way of verifying the materials reported in journal articles. Now there is no way of proving that these authors fudged their publications but in a suspicious frame of mind, one can wonder. Incidentally, a decade after Wolins, something of the same result appeared in a replication (Craig & Reese 1973). But these reports did not produce a scandal in psychology because this sort of fudging is, in fact, well known. Scientists know the many games of publishing. For example, in their study of a physics department, Mulkay and Williams (1971) report:

> *All* our respondents thought the vast majority of papers in the journals which they read were of poor quality or of little significance. One respondent stated that 'only 2% of publications are significant and 50% are wrong' (emphasis in original). (p. 74)

In his masterful review of mathematical literature, Kenneth May reports that fully 43% of the articles which were published in a particular sub-speciality were "trivia." He defined trivia as "You'll grudge the time reading it, even if very interested in the subject" (May 1968:366). Mathematicians know that trivia abounds in their literature and this is not a scandal because publications are not only abstract contributions to science; publications have purposes besides scientific universalism and "everybody knows it." Publishing is just another game in academe and it is just one way of managing to "look good" in science. Understood as a way of padding one's *curriculum vita*, there is not real harm done and there is nothing *new in any of this* to the scientific insider.

Scientists know the *games of science*. They are all aware of trivial publications, fudged data, edifice complexes and outright theft. There is nothing new to the scientific insider here. But scientists do not admit these sorts of things to the laity. As Mulkay (1979) suggests:

> scientists enter the political context as purveyors of certified knowledge. They have nothing to offer other than the supposed certainties of science; and if they were to present their conclusions as no more than plausible guesses, based on uncertain foundations, they would carry little practical weight. (p. 117)

Scientists build and maintain images for "the political context...", for outsiders from whom scientists derive social benefits of various kinds. But creating and maintaining images for external consumption are the jobs of "sages," in Znaniecki's use of that term (1968), although I prefer the more descriptive term "science watchers." *The science watcher is here defined as one who constructs and maintains the images of science, one who is responsible for the public relations of science and other human institutions.*

Science Watchers' Games

Fudge, fraud, deceit and deception are easily found in histories and contemporary studies of science. If one looks for cheating in science, it is easily found. Indeed, science watchers have been unable to avoid finding evidences of dishonesties in the history of science and they have, therefore, developed various techniques for handling this "seamy side of science." James Conant (1951)--chemist, scientific historian, politician and administrator--provides good examples of what is called the "discounting game" of scientific cheats. For example, Conant writes of the Father of Modern Chemistry, Anton Lavoisier, as if Lavoisier were a hapless victim of his times rather than a gamesman of science. Lavoisier had drafted a paper on gases in 1776 which contained gross errors. Fortunately for him, Lavoisier met the English chemist, Joseph Priestley, before the error-ridden paper was published. Priestley provided Lavoisier with the hints necessary to amend the article. The official version, however, contains no reference to Priestley; Lavoisier does not acknowledge the assistance he had been given. Lavoisier pretends he did the work by himself, by stealing an idea. To this Conant (1951) comments:

> (With Priestley's corrections)...he was able to correct the first version by a few deft strokes of the pen, and as finally printed in official form in 1778 this 'classic' paper gives no hint either of his misstep or of Priestley's unsolicitied assistance. The ethics of science have changed since the eighteenth century. Today an investigator would be scrupulous about acknowledging both oral communication and prior publications (emphasis supplied). (p. 188)

Elsewhere (Conant & Nash 1957) Conant further details Lavoisier's theft of Priestley's ideas and explores Lavoisier's rationalizations of the theft. After 1778, Lavoisier claimed that he had discerned Priestley's hints which led to his famous paper as early as 1772. Conant comments on this lie: "(Lavoisier) never reported on his 1772 experiments which were in part at least, erroneous" (Conant & Nash 1957:73). Conant has clear evidence that Lavoisier is a liar and a thief but he simply suggests that the lying was "erroneous" and the theft would not happen today. Clearly, it is not consonant to have "The Father of Modern Chemistry" thought of as a liar and a thief, so this historian of science "discounts" these behaviors.

Conant provides other examples of his discountings of fraud in the history of science. Regarding Pascal's experiments with the barometer, Conant has it that Pascal had reasoned that a column of mercury should be longer at the base of a mountain and shorter at the top. The experiment was performed in 1648 by Pascal's brother-in-law and was successful. Indeed, it was spectacularly successful in terms of the reported measurements taken on a blustery fall day. Pascal reports that his colleagues were able to report measurements accurate to one twenty-fourth of an inch. Conant (Conant & Nash 1957) comments:

> One cannot help, however, but be somewhat skeptical of

> the high degree of accuracy reported... To be able to repeat the Torricellian experiment so that there was less than a twelfth of an inch...difference in successive readings, as claimed, is remarkable... This report...was written it must be remembered, before standards of accurate reporting in science had been established. It may be that (Pascal) succumbed to the temptation of making his argument appear convincing (emphasis supplied). (p. 8-9)

Pascal's is a clear case of "cooking" data, as Babbage called such "improvements" in reports (Babbage 1969:175-183). Pascal is guilty of making his data "look good," of establishing a degree of accuracy that was impossible. Here is clear evidence that like scientists to come later, early scientists tried to make themselves "look good." Clearly, trying to look good in science--and in other spheres of human activity--is nothing new. But how does Conant handle these fabrications of data? He suggests that this is, again, a case of scientific adolescence, that "standards" would develop as science progressed. There is certainly no outrage in Conant's version.

The point here is this: Pascal and Lavoisier tried to make themselves look good and James Conant, the science watcher, is trying to do the same sort of thing. Pascal and Lavoisier promoted themselves, while Conant promotes what Lavoisier and Pascal stood for, science. Conant, who is in a position to judge the perfidies of Lavoisier and Pascal, behaves as an apologist for fudge, fraud, deceit and deception. There is a parallelism here: some scientists cheat directly while some science watchers cheat indirectly by discounting, at least, some forms of cheating. Some science watchers are guilty of deception too in that they try to make science into what it is supposed to be. In this, both scientists and science watchers may be viewed as gamesmen.

The ploys used by science watchers are not limited to discountings. Science watchers have a variety of techniques with which to maintain the image of science in spite of obvious deceptions. For example, Asimov's The New Intelligent Man's Guide to Science beclouds the issue neatly. Recall that Lavoisier's theft and mendacity were matters of dates: 1776, 1778, and 1772. Asimov (1965) informs his "intelligent" reader simply "In experiments conducted in the 1770's..." (Asimov 1965:157) and the whole issue is sloughed off.

Douglas McKie (1959) goes so far as to call Lavoisier the "Newton of Chemistry" and he does mention that Lavoisier met Priestley, but McKie avoids any implication of impropriety. It is, after all, unseemly--to say the least--to have our Newtons described as having made their ways by cribbings. The sage remains silent and the centrality of theft by Lavoisier in the development of modern chemistry is hidden.

John White's (1932) sympathetic treatment of early chemistry as a history which is very conscious of the social influences which produced the shift from phlogiston theory, quite simply avoids any implication that Lavoisier stole Priestley's ideas in writing the famous memoir.

Bernard Jaffe's Crucibles does mention the omission of references to Priestley's work and even cites a letter of Priestley's complaining of Lavoisier's neglect. Jaffe adds, "It is difficult to explain this omission..." (Jaffe 1976:78-79). I argue that it is not difficult to explain at all unless one is unwilling to report the fact that Lavoisier cribbed Priestley's ideas. And that seems uppermost in the mind of Jaffe, the science watcher.

Thomas Kuhn, Conant's student, is also kind to Pascal (Kuhn 1977:44-45) and writes only that Pascal's work was part of a tradition of "mental experiments" which sufficed as "good science." In other words, this historian of scientific revolution is not so much interested in the tactics of scientific revolutionaries so much as he is in "explaining" data faking.

One should not be misled into thinking that discounting a la Conant and these few others is the only technique of the trade of science watching. Daniel Greenberg (1981) used humor very effectively in describing/disguising the games played by scientists. Stephen Jay Gould (1977, 1978) manages to explore the particularisms of biology and psychology using parody and hyperbole. Stephen Bush (1974) has managed to recite some of the sins of science by "reserving" the information to "mature readers." C. P. Snow (1961) has dramatized the possibilities of cheating in science by writing "fiction." But Snow and the Cambridge scientists who worked with him, knew the power of good fiction. Snow (1967) quotes Rutherford's reaction to his first novel:

> 'It's a small world, you know,' he meant the world of science. 'Keep off us as much as you can. People are bound to think you are getting at some of us. And I suppose we've all got things we don't want anyone to see.' (p. 8-9)

These ploys of science watchers--telling the truth without telling it--are all forms of image-maintenance to outsiders and examples to insiders.

It is unnecessary to extend this list of cheats in science to make the point that finding examples of famous cheats in science is easy. Cheating in science is not uncommon. What is new here is not that scientists cheat, but that science watchers persist in their image maintenance for science. Here is the major shift accomplished by this interpretive approach: one begins by trying to understand fudge, fraud, deceit and deception committed by scientists, and quickly finds that the context of understanding fraud and fudge is not clear. It is necessary to understand the interpreters of science, the science watchers. There is a new game to be played: interpreting the interpreters.

In their tellings and retellings of the stories of Great Science, science watchers persist in disguising the crafty paths to truth. And there is this lovely paradox at the center of all this: it is the very pretense to Great Science which becomes the barrier to our understanding of what really goes on in science. Our own best efforts to look good are the tragic flaws in our efforts to understand ourselves.

The Manufacture of Villains

It is not only in hero construction that the science watchers play their games. Not merely do science watchers discount misdeeds, they are also responsible for the creation of villains, the negative referents for vile science. It is the science watcher who pontificates at the occasional, but necessary, auto da fe.

Legend has it that Galileo was demonstrating his law of uniform acceleration at the Leaning Tower of Pisa as early as 1591. The tale is a myth (Cooper 1935) and, in fact, Galileo wrestled with that problem until late in his life. His "law" took him forty years to perfect and any demonstrations of its utility during his lifetime are suspect (Koyre 1953, 1968; Settle 1961). Indeed, American astronauts took time on the moon to film their own demonstration of its accuracy and their investment is remarkable testimony that Galileo could never have convinced "Doubting Thomists" by any demonstration in his day. Rather, Galileo employed "mental experiments" and rational arguments in describing what would happen when objects fall.

The case is clear: Galileo never performed experiments he claimed to have done, he reported results he never obtained, and he stole ideas from other people outrageously. Paul Feyerabend (1975) has suggested that Galileo was forced to fudge in order to nudge truth along. As a true revolutionary, Galileo had to propagandize successfully if he were to succeed in the promotion of modern science. Generalizing from the case of Galileo, Feyerabend suggests that propagandizing may be necessary in science if scientific revolutions are to succeed tactically and strategically. Feyerabend as a science watcher suggests that the scientist must be a heretic-revolutionary if he is to succeed.

Arthur Koestler (1959) goes further, suggesting that Galileo was a mean-spirited thief who cribbed from Kepler and the Jesuit astronomers of his day, shamelessly failing to credit his sources. Alexandre Koyre (1958, 1968) similarly tells tales of Galileo's deceptiveness: Galileo's reliance on mental experiments, his cribbings from Kepler and others, and his abuse of science for political purposes.

There are, then, some good reasons to think that Galileo could be regarded as a miscreant in science but, rather, he is judged a hero. It seems quite clear that this stature is an ascription rather than a description. What Galileo did, and did not do, is subject to the interpretations of science watchers who credit Galileo with a variety of talents. One can see that it could have gone the other way and that Galileo, for doing exactly what he did, could have been interpreted a cheat in science. The question is not whether Galileo should or should not be regarded as a hero in science but, rather, "Why did science watchers choose to make Galileo the hero he has become?" And we do not know the answer. Yet this becomes even more intriguing when we consider some of the other examples of this mysterious process.

Westfall (1974, 1980), for example, suggested that Isaac Newton fudged his arithmetic and achieved a preposterous degree of accuracy by cooking his data. Newton apparently succumbed to the temptation of making his argument appear convincing. But there is more to Newton's unscrupulousness than just cooking data. In his manipulations of the Royal Society he was positively Machiavellian in his destruction of Leibniz. It is clear that he stacked the commission of the Royal Society charged with examining the priority dispute between Newton and Leibniz concerning the invention of the calculus. He abused his Presidency of the Royal Society by commanding personal loyalty in exchange for academic appointments throughout the British Isles. His insistence on chauvinistic, English notation (i.e., Newtonian) undoubtedly set back British mathematics for a century (Hall 1980; Manuel 1968). But in spite of all this, Newton is regarded as a hero in science.

Mysterious, then, are some of the ways in which heroes can be created. That is, science watchers simply interpret and reinterpret whatever has been done and, voila, instant hero. However, this much should be reasonably clear: *heroes in science are doing the same sorts of things as are the presumptive villains of science.* Both heroes and villains depart from universalism, yet heroes are heroes and villains are villains. *Heroism and villainy are ascriptions rather than achievements. One is accorded the status of a hero or a villain not by effort alone but by being labeled a hero or a villain by science watchers.* If one is to understand this process, it is imperative to understand science watchers because they are the action in the field.

Contemporary Revisionism

With the suspicion that villains and heroes are in large part the creation of science watchers, it is possible to reexamine the "modern cheats" to see what they represent in the 1980s. A major scandal of the 1970s was the case of William Summerlin, a cancer researcher at Sloan-Kettering. There is no doubt that Summerlin inked some tissue-grafts on mice so as to make it appear that he had been successful in suppressing immunological reactions to transplants (Hixson 1976). It was a fraud and he confessed to his superior, colleague, mentor and erstwhile collaborator, Dr. Robert A. Good, Director of Sloan-Kettering. His confession led to a review of all of his work and the disgrace of being caught as a cheat. He was fired at Sloan-Kettering. On the face of it, the Summerlin case is a clear example of the effective operation of social controls in science: cheats get caught and disgraced.

There are, however, some disquieting things about the Summerlin affair. For one thing, Summerlin and Good were collaborators in research and had co-authored several articles. One might reasonably expect that partners would share in the disgrace. Quite the contrary, Culliton reports that the review committee of the Institute treated Dr. Good "ever so gently..." (Culliton 1974a, 1974b:1157) and quotes informants at Sloan-Kettering: "Good is too valuable a scientist. We can't let him be dragged down by this" (Culliton 1974b:1157). Apparently, Institute Directors are too important to be labeled cheats while associate professors are not. Here, it would seem, cheating is to be understood in terms of social status.

Another nettlesome point is that Summerlin got off quite easy. The President of the Institute, Lewis Thomas, is quoted (Culliton 1974b) as saying:

> it has been agreed that the Center will provide Dr. Summerlin with a period of medical leave on full salary ($40,000) beginning now, for up to 1 year, to enable him to obtain the rest and professional care which his condition may require (emphasis supplied). (p. 1555)

This is, indeed, something of an uneasy resolution to a case of scientific fraud. Were fraud really the heinous sin of science, and Summerlin a confessed fraud, these niceties would be an outrage. One guilty of abominations is not given medical leave, at full salary, for rest and professional care. Summerlin, driven from research, was allowed to return to the practice of medicine where, as is said, he can bury his mistakes. In truth, it does not appear that this sort of "punishment" fits any "crime" at all. Much may be made of the social control mechanisms whereby science polices itself, but in this case, there is no evidence that science severely punishes those who violate its universalism.

Some other images of science's social control appear to be clearer. In 1927, for example, Paul Kammerer, a biologist, was accused of fraud (coincidentally, inking a specimen) and, shortly thereafter, killed himself. Concerning Kammerer, Aronson (1975) writes:

> I was familiar with this amazing story, for it was regularly told to biology students as an object lesson. In essence, falsifying evidence is just about the worst sin that a scientist can commit since such actions threaten to destroy the very heart of the scientific system. (p. 115)

Those who would destroy the heart of the scientific system are not given sick leave at full pay and allowed to retire to the practice of medicine--or so students are told in their lessons. Kammerer once symbolized that fraud in science is suicidal--and that is powerful imagery.

But there is something wrong, even here. Aronson's quote, above, is taken from his review of Arthur Koestler's biography of Paul Kammerer, The Case of the Midwife Toad (1971). Aronson dislikes Koestler's thesis which is, put bluntly, that Kammerer was the victim of professional in-fighting among biologists. Kammerer's being discredited--his suicide--signaled the demise of Lamarckianism and the apparent victory of neo-Darwinism. To Koestler, Kammerer was not a scientific fraud but a human sacrifice to professional in-fighting and Aronson is simply trying to discount the Koestler thesis. Aronson's major point is that young professionals, and the laity, ought to be aware of the popularizers of pseudo-science, like Arthur Koestler. One science watcher's hero is another's fraud. It really seems to depend on who is doing the interpreting.

Another model of effective social control in science is that of Rene Blondlot, the French physicist, who in 1903 reported the discovery of n-rays, and was much rewarded by the French government for his work. In 1904, the American physicist R. W. Wood exposed Blondlot as a victim of self-deception. The unkind exposure of Blondlot in *Nature* (Wood 1904) was presumptively followed by ignominy and death (Langmuir 1968; Rostand 1960). The image of Blondlot is supposed to be quite clear: do not be made the fool.

However, Professor Blondlot did not die in disgrace but survived quietly, until 1930 (Klotz 1980). Furthermore, the Frenchman was "exposed" only after Wood had been egged on--more or less goaded into it--at a meeting of the British Association of the Advancement of Science. It is true that the American had a reputation for exposing frauds; he liked using his skills in debunking mediums and parapsychologists (Seabrook 1941). But it could be argued that this case represents a chauvinistic particularism in science, at least since Newton, the French, the British and the Germans have politicized science for their respective national prides. Blondlot may represent nothing more than a tactical victory for the Germans and the British in that continuing war.

Blondlot, according to this reinterpretation, is a victim of political warfare in science--scientific nationalism which belies the claims of science to universalism. Blondlot is, therefore, a poor example, a poor object lesson, to hold up to students. Blondlot's real stupidity was that he did not see the American as a potential threat, a British agent, a scientific gamesperson bent on embarrassing the French. There are lessons to be learned from Blondlot's case including this: keep laboratory doors locked in that "we've all got things we don't want anyone to see." Blondlot is evidence that scientists ought to be at least sometimes suspicious of one another.

Such an examination of heroes and villains as the above in science leads to this point: *in order to understand heroism or cheating in science it is necessary to examine the science watchers who label heroes and villains.* The labeled cannot inform us why they have been labeled. One can ask, What do the science watchers derive from their assignment of labels? Or, What does an interpretation profit those who make the interpretation? It is in terms of science watchers that the answer must be given. Their interpretations must be looked at as if they were to the benefit of those using them. With this in mind, it is possible to reexamine the spate of contemporary cheats of whom so much has recently been made. One can now ask what this handwringing is all about.

The few cases of recent cheats--from Alsabti through Soman and Felig--can be used as evidence that the system of social control in science is working. In this *the publicizing of a few recent science scandals may be self-serving.* These few cases are purported demonstrations that science is, in fact, as it is supposed to be, self-correcting. These few cases of cheats, dragged out whenever evidence may be required, are what passes for evidence of the adequacy of science's own policing methods. These cases may, however, be differently interpreted.

By focusing on specific areas of research in which frauds have occurred, other areas of science are left virtually unexamined and untouched. Most of the cheats cited earlier are engaged in medical research sponsored by the federal government. These few scapegoats seem positively heavensent in the lost War on Cancer as there is not another area in contemporary research in which the taxpayers' need of confidence is greater. Evidence of any professional self-control is reassuring. Moreover, with these few scapegoats, Defense Department R & D looks positively clean. And university administrators can persist in whatever passes for control of research so long as they can wring their hands over, say, Summerlin's inked mice.

Third, excepting the Burt case, medical personnel are vastly overrepresented on the list of cheats. It could be argued that physicians are being used to represent misdeeds in science. This may be academic gamesmanship at its worst: it may be an attempt to get even with physicians whose salaries and social status are higher than those of other academics.

And, finally, the case of Sir Cyril Burt is felicitous for its symbolic value: Burt's censure is useful because his work had been used to support an unpopular, hereditarian position in psychology. In finding Burt a cheat, environmentalists have created a useful straw man.

These four "interpretations" are offered as alternatives to the more customary image-maintenance functions engaged in by the sages of science. They are meant to be provocative in that they suggest that there are "things" going on among science watchers. The contemporary freshet of exposed cheats in science is, then, something of a fabrication, a science watcher's ploy. This does not mean that the accused are "innocent" of the "crimes" of which they are accused; the evidence against Burt (Dorfman 1978; Hearnshaw 1979), for example, is convincing. Rather, it is in their pretended indignation that contemporary science watchers betray their colors (Hunt 1981). My whole point is that there is nothing new to cheating in science and there is nothing new to the pretenses of science watching.

There are, as Steffens (1958) suggested in his Autobiography, good reasons to cheat in science. The task of the sage of science is to cover up those reasons, to pretend that cheating does not happen save for the few rotten apples in the otherwise healthy barrel. In this, the science watchers conveniently rewrite the history of science. For the sages of science are ultimately loyalists who see the games of science played according to the rules of fraternity; there is no whistleblowing.

There are steep costs to this fraternal organization of scientists and science watchers, not the least of which derive from the self-imposed blinders science watchers wear. There are, too, the costs borne by fledgling scientists who must learn the subtle rules of gaming without being told that gaming is going on; in this, each individual must learn by himself just how to play the games of science. How much more reasonable would it be to instruct tyros on the rules of the scientific games.

And finally, there is a gambit used by some which should be mentioned in any appeal to air the games of science. It is this: there is a logical objection to observing science watchers because there should be someone observing the observers and so *ad infinitum*. This is just another ploy for continuing the old games of science and it is time for a change.

CONCLUSION

Fraud, fudge, deceit and deception are quite common in science. The current spate of cheats ought to be looked at in this light and interpreted in terms of the functions they serve as symbols of "effective social controls in science."

Not only do scientists play the games of science but there is another game worthy of study: the game of science watching whereby science maintains its image. There is a need to study the sages of science if we are to understand science itself.

Finally, it should be clear from the context, that any new policies of federal agencies to beef up the control mechanisms of contemporary science are to be counted as inept until there are some efforts to understand the games being played in the name of science.

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FRAUDS FROM 1960 TO THE PRESENT: BAD APPLES OR A BAD BARREL?

William J. Broad
The New York Times

There are at least 20 well-known cases of scientific fraud for the period between 1960 and the present. They range from outright plagiary on a grand scale to the wholesale invention of data in the laboratory of a Nobel Laureate. I would like to review some of these cases, and, in a *journalistic way*, speculate on what the phenomenon of fraud can tell us about the way science works in everyday practice.

According to the conventional wisdom, science is a strictly logical process, objectivity is the essence of the scientist's attitude to his work, and scientific claims are rigorously checked by peer scrutiny and the replication of experiments. From this self-verifying system, error of all sorts is speedily and inexorably cast out.

My colleague Nicholas Wade and I began to doubt this view in the course of reporting some of the recent cases in which scientists had been discovered publishing results that were fictitious. At first we examined these episodes of scientific fraud in terms of individual psychology: how could a researcher, committed to discovering the truth, betray the central principle of his profession by publishing false data? We were influenced by the spokesmen for the conventional ideology of science, who invariably stressed the individual nature of the crime. The falsification of data was the product of a deranged mind, they would say; it had been discovered, as was inevitable, by the self-policing mechanisms of science, and there was nothing more to worry about.

As more cases of fraud broke into public view, and whispers were heard of others more quietly disposed of, we wondered if fraud wasn't a quite regular minor feature of the scientific landscape. We also noticed, upon closer examination, that the cases failed to conform with the model of science implied by the conventional wisdom. Logic, replication, peer review, objectivity--all had been successfully defied by the scientific forgers, often for extended periods of time. How had they managed to get so far for so long? And if fraud was as surely doomed to failure as the official spokesmen proclaimed, why did so many people try it?

Since fraud had led us to doubt the conventional ideology, we eventually came to believe that it might also furnish a useful alternative perspective for looking at science. The conventional ideology, in our view, derived from the work of historians, philosophers and sociologists who had looked at science not for its own sake but from the perspectives of their own disciplines. Like some travellers to a foreign land, these professional observers of science have learned more about themselves than the country they visited. Why should fraud not offer another route to understanding? Medicine, after all, has derived much useful knowledge about the normal functioning of the body from the study of its pathology.

By studying science through its pathology, not through some preconceived criterion, it is easier, we found, to see the process as it is, as distinct from how it ought to be. Cases of fraud provide telling evidence not just about how well the checking systems of science work in practice, but also about the fundamental nature of science--about the scientific method, about the relation of fact to theory, about the motives and attitudes of scientists.

Our conclusion, published in the spring of 1983 by Simon & Schuster, Betrayers of the Truth, is that science bears little resemblance to its conventional portrait. (References to the various cases and quotes that appear in this journalistic review may be found in Betrayers.) We find that the logical structure discernible in scientific knowledge says nothing about the process by which the structure was built or the mentality of the builders. In the acquisition of new knowledge, scientists are not guided by logic and objectivity alone, but also by such non-rational factors as rhetoric, propaganda, and personal prejudice.

Thus, despite the widespread faith among scientists in the self-correcting nature of science, there is evidence that fraud often is not automatically cast out of the corpus of science, and that, in fact, the phenomenon of fraud may be on the rise. The problem may not be with bad apples that occasionally roll into view, but with a bad barrel.

One reason is opportunity: the growth of large elites at institutions such as Harvard or Yale offers data fabricators a certain immunity from scrutiny. In addition, the system of scientific communication has become so cluttered with meaningless papers and journals that frauds have little fear of detection. Another reason is that the careerist nature of contemporary science encourages the creation of frauds. The race for priority, grant renewals, tenure, and quick success may tempt a researcher to cheat, as does the changing nature of the scientific workplace. A biomedical lab chief may claim 500 papers, but the bulk of the work is done by juniors. The dislocation between work and reward can result in the breakdown of the relationship between master and apprentice, turning labs into a breeding ground for cynicism and outright fraud.

In short, an examination of some of the 20 well-known cases that have emerged in the years after 1960 shows how the actual workings of the scientific enterprise are at times very different from those described by defenders of the conventional ideology.

A notable failure of scientific self-policing is seen in the extraordinary case of Sir Cyril Burt. One of the pioneers of applied psychology in England, Burt was a man of brilliance and great culture. He became professor of psychology at University College, London, and was the first psychologist to receive a knighthood for his services. The American Psychological Association gave him its Thorndike Prize in 1971, the first time that the high honor had been awarded to a foreigner. When he died, the same year, the obituaries proclaimed him

"Britain's most eminent educational psychologist," and even "dean of the world's psychologists." "Everything about the man," wrote Arthur Jensen of Stanford University, "--his fine, sturdy appearance; his aura of vitality; his urbane manner; his unflagging enthusiasm for research, analysis and criticism;...and, of course, especially his notably sharp intellect and vast erudition--all together leave a total impression of immense quality, of a born nobleman."

But the man who impressed Jensen with his nobility of intellect possessed a grievous intellectual flaw: he was a cheat. He invented data out of whole cloth to support his own theories and confound his critics. Burt's chief work, a study of intelligence based on separated identical twins, emphasized a high degree of heritability. It was not merely wrong; it was riddled with glaring statistical errors, later shown to be a sign of widescale fraud. Since it purported to be the largest collection of IQ data of its kind, it was repeatedly cited by both proponents and critics of the hereditarian view. Yet throughout the heat of the debate, the scientists who rested their case on Burt's data failed to spot its flagrant inconsistencies. Perhaps even more remarkably, the critics of Burt's position also missed the red flags that leapt out from almost every paragraph of his disputed writings.

Burt attained a great part of his eminence in the field of IQ testing not because of any thoroughgoing program of research, of which he did little worth the name, but through his skills of rhetoric. If a real scientist is one who wants to discover the truth, Burt was no scientist, because he already knew the truth. He used the scientific method with great effect, but not as an approach to understanding the world. In Burt's hands the scientific method can be seen most clearly for its utility as a purely rhetorical device, a method of argument with which to assume a position of moral superiority, to pretend to greater learning or diligence.

Burt did not base his findings on new data or the efforts of his co-workers. The lonely and embattled Burt, sitting in his armchair, summoned both data and co-workers from the deep of his tormented imagination, and clothed them so well in the semblance of scientific argument that the illusion fooled all his fellow scientists for something on the order of 30 years.

One observer of the Burt episode, Leon Kamin of Princeton University, summed up the situation this way: "Every professor knew that his child was brighter than the ditch-digger's child, so what was there to challenge?"

Another triumph of belief over skepticism is seen in the remarkable case of Mark Spector, a 24-year-old graduate student at Cornell University who in 1981 trapped many scientists in the coils of his work. A single attempt at replication by the many oncologists who were fascinated by Spector's theory of cancer causation would have stopped the fraud in its tracks. Yet for a long time the simple human factors that often shape scientists' attitudes and motivations pushed out of mind the basic methodological safeguard of replication. During that period, Spector's theory spread its web over the whole field of cancer research, tangling several leading scientists in its threads.

While the mechanisms that might deter fakery are often illusory, the temptations facing a researcher in the fast-paced world of contemporary research are quite concrete. Truth no doubt remains the primary goal of most scientists. For many, however, a more immediate objective is to establish credit--often defined as a long list of publications --which helps in the continual struggle to secure government grants and win academic promotion. Since few scientists, much less administrators, have time to read these articles, the quantity of scientific papers listed on a curriculum vitae can often be more important than their quality.

One effect of the publishing game is an extraordinary expansion of the literature. According to the British Medical Journal, there are currently in the world 8000 journals devoted to biology and medicine alone. The clutter has hindered the checking mechanisms of science and lessened the deterrent for would-be cheats and plagiarists. According to a famous 1942 essay written by Robert K. Merton, the father of the sociology of science, "Scientific inquiry is in effect subject to rigorous policing, to a degree perhaps unparalleled in any other field of human activity." This occurs, according to Merton, through "organized skepticism," the critical review of published work by the scientific community at large. Organized skepticism may have worked well in 1942, but not today.

Its failure can be seen in the case of Alias A. K. Alsabti, a plagiarist of middle eastern extraction who between 1977 and 1980 lifted many, perhaps all, of his 60 published papers. His goal, like that of many scientists, was to further his career by compiling a long list of published papers. Alsabti worked at the extreme edge of the U.S. research establishment, publishing stolen work with impunity in seldom-read journals. Yet it took three years for the community of scholars to detect and halt his unprecedented abuse. Lesser cheats might never be stopped.

The Alsabti affair sheds light not only on the careerist tendencies that pervade the research enterprise, but also on many of the basic internal mechanisms of contemporary science. The exploits of Alsabti could never have occurred in a community of scientists where rigorous self-policing was the rule, and instant expulsion was the automatic penalty for any form of dishonesty. Even when his methods eventually came to light, fellow researchers were reluctant to make a public issue of his cheating. Alsabti would be allowed to leave quietly and would find a job in another laboratory where the same process would start over again. It was only after Alsabti's exploits were described in a handful of international journals that the career of this middle-eastern plagiarist came to a halt.

A more subtle way of expanding one's share of credit in the publishing game is to coauthor many papers. A serious type of coauthor abuse, for example, is seen in the large lab where a senior scientist provides little work or inspiration but manages nonetheless to walk away with a large measure of the credit for the efforts of his underlings. Today, it is not uncommon for the name of a prominent biomedical lab chief to appear on 500 or 600 papers produced in large measure by his juniors. A disheartening aspect of this publishing game is the impact the research mill sometimes has on the attitude of young researchers. The dislocation between work and reward makes large labs a breeding ground for cynicism--an atmosphere that encourages finagling of data or wholesale invention of results.

An example comes from the case of Robert Gullis, a young doctoral candidate who worked between 1971 and 1974 at Birmingham University in England. A few years later, Gullis admitted that he had fudged most of his work. With, undoubtedly, a self-serving element to his remarks, Gullis said he was the victim of a system gone bad. "It's a problem you face everywhere," he told a reporter. "Not once was I ever rewarded for my work. They were very happy with the results. They were just after results and were quite glad when someone was there working really hard to get them out and a Ph.D. at the same time." Gullis retracted 11 papers. Seven of them had been coauthored with his senior professor, who, by the way, was not the one who discovered the fakery.

The same sort of research-mill cynicism would seem to apply to William Summerlin, the protege of Robert Good who worked at Sloan-Kettering Institute for Cancer Research and gained immortality in the scientific hall of shame by painting the skin grafts of some mice. "My error," said Summerlin, "was not in knowingly promulgating false data, but rather in succumbing to extreme pressure placed on me by the Institute director to publicize information..." Summerlin coauthored many of his papers with immunologist Robert Good, the director, who in a 5-year period published almost 700 scientific reports, a feat achieved by dint of hard work and establishing an empire of research workers under his personal banner.

Another remarkable case of a paper factory in action came to light in 1978 at Boston University, where not one but a whole group of young researchers in a 40-member cancer research team claimed they had been pressured into falsifying data and records by the team leader, Marc J. Straus. Straus, 34, was a driving, intense research physician who specialized in lung cancer. He was highly successful in attracting private and federal funding for his work, being awarded nearly $1 million over a three-year period. He had published more than 40 articles and a book, had organized a half-dozen cancer clinics in affiliated hospitals outside Boston, and had achieved a national reputation. One day in the spring of 1978, Straus sat with some of his staff in a 14th floor classroom at Boston University Medical Center and daydreamed out loud about winning the Nobel Prize. Less than three months after the Nobel musings, however, the structure on which Straus had built his empire suddenly collapsed. Five members of his staff, including two young physicians on fel-lowships and three nurses, went to officials at Boston University and said the team's reports contained numerous lies. The falsifications ranged from simply changing a patient's birthdate here and there to reporting treatments and laboratory studies that were never done and inventing the existence of a tumor in one patient. The five charged that the falsifications were made under Straus' orders. But others on Straus' staff said the fictions were perpetrated amid a general anxiety that a shortage of patients might threaten future funding for their research. Straus resigned under pressure, insisting that he did not know of the falsifications and that he had been the victim of a conspiracy.

Nearly four years after the tempest broke, the Straus case ended in the first use of federal "debarment" regulations by which a researcher caught in the midst of an unethical situation can be cut off from the federal research pipeline. On 17 May 1982, Straus signed an agreement with the National Institutes of Health and the Food and Drug Administration whereby he admitted that he was responsible for work submitted to federally funded programs that contained fabricated data on how patients responded to a complex regime meant to combat lung cancer. The debarment means that Straus, for a period of four years, will be unable to study investigational new drugs and will be unable to receive any form of financial assistance from the Department of Health and Human Services. Though not legally taking responsibility for the false data, Straus strongly maintains that he had no part in the falsifications and was victim of a conspiracy: "I have never been party to any data frauds or manipulations...and nothing in the settlement with the government indicates that I was such a party."

If labs have evolved an atmosphere conducive of fraud, so too, the growth of large elites at prestigious universities seems to have created an atmosphere in which frauds go unrecognized because the institutions are so highly regarded. Thus, the work of a well placed cheat is often rendered a certain immunity from scrutiny. For example, in 1979, when a call was made by a young researcher for an investigation into the work of two biomedical researchers at Yale University, Vijay Soman and Philip Felig, the plea went unheeded. One auditor from the National Institutes of Health never got around to looking into the case because, as he put it, "I just found it hard to believe that Felig had engaged in any hanky panky." It took a year and a half of threats, letter-writing, and late night phone calls before the young researcher had the satisfaction of seeing an investigation launched and the fraud found out. In the end, Soman admitted he had faked certain results and soon left Yale. Twelve of his papers were eventually retracted. His boss and coauthor, Felig, was thrown into an administrative and ethical tangle. Felig eventually lost the job he had gone onto as chairman of the department of medicine at the prestigious Columbia College of Physicians and Surgeons.

Belief in the conventional ideology can wax so strong that widespread fakery will at times be ignored, simply because it is "impossible." One such case recently unraveled at the very heart of the American biomedical establishment, the Harvard Medical School. The Harvard authorities, confident in the self-policing mechanisms of science, could not see the dimensions of the problem unfolding right before their eyes.

The crux of the Harvard case was that Eugene Braunwald, physician-in-chief for two of Harvard's most prestigious hospitals and one of the country's leading cardiologists, held nothing but the highest hopes for his brilliant young protege, John Roland Darsee. Tall and affable, the young physician worked relentlessly at the cutting edge of cardiovascular research. In two years at Harvard, Darsee published nearly 100 papers and abstracts, a phenomenal number by any standard, and many of them jointly authored with his mentor Braunwald. Braunwald, who presided over two separate research laboratories and more than $3 million in funds from the National Institutes of Health (NIH), was considering setting up a separate lab for Darsee at Harvard's Beth Israel Hospital. In the competitive world of Boston biomedical research, a promotion of this kind, at Darsee's early age, would ensure a dazzling career.

Darsee was less highly regarded by the other young researchers in Braunwald's laboratory. Hard though Darsee worked, they could not understand how he did all the research on which his prodigious number of scientific articles were based. Secretly observing him one evening in May 1981, they saw him flagrantly forge the raw data for an experiment that was shortly to be published. Darsee confessed when confronted, but insisted that that was the only experiment he had ever faked. His colleagues were less inclined to agree. By comparing his actual experiments with what he later wrote up for publication, they decided that much of the work had been created out of whole cloth. "This thing did not come up over night," remarked one of Darsee's colleagues: "You suspect it for months." They told Braunwald they believed Darsee was systematically faking his work.

But Braunwald could not believe the troubling event was anything more than an isolated incident. "At the moment we had a brilliant person," he later remarked: "He clearly was one of the most outstanding, or the most outstanding, of the 130 research fellows I have been privileged to work with. Public disclosure would have ruined him for life." Darsee was stripped of his Harvard appointment, but allowed to go on working in the laboratory. Other researchers were not told of the faked experiment, nor were any steps taken to inform scientists who might be depending on Darsee's numerous published results that a question mark hung over the whole corpus.

Darsee stayed on in Braunwald's laboratory, doing research and publishing papers as if everything were just fine. Among the experiments he worked on was a $724,154 project funded by the National Institutes of Health (NIH). For five months everything continued as before. But in October 1981 the Harvard authorities were told by an official at NIH that there were problems with the data which Darsee had submitted. Only then did they begin to realize that a researcher who had forged one experiment might possibly be tempted to fabricate others.

A blue-ribbon committee appointed by the dean of the Harvard Medical School confirmed three months later that the study done for NIH contained "unusual results which are highly suspect." Furthermore, a study undertaken by Darsee with another researcher appeared "to have been manipulated." Darsee denied any improprieties except for the original fakery in May. The blue-ribbon committee, made up largely of senior medical officials, could see little wrong with the way their colleagues at Harvard had handled the affair, but a senior NIH official chided Harvard on national television for the delay in reporting the incident of fraud. A year after Darsee had been caught red-handed in fabricating data, the extent of fraud in his published work had still not been assessed and publicly pronounced upon by the Harvard Medical School authorities.

The actions of Harvard officials during the half-year unfolding of the affair seem to epitomize the argument made by Handler at the congressional hearing. Scientific fraud happens rarely, and when it does, "it occurs in a system that operates in an effective, democratic and self-correcting mode" that makes detection inevitable; anyone who tries to fake scientific data, goes this argument, must be crazy. Since Darsee was clearly a rational person with a bright future, Harvard officials could only see his admitted fabrication as an isolated aberration, not as part of a general pattern. The chance of Darsee cheating in the NIH study after he'd already been caught once, Braunwald said later, was "vanishingly low."

In sum, the individual cases of fraud that have come to light during the past two decades are not less interesting in themselves as what they say about the scientific process. Science, it would seem, often bears little resemblance to its conventional portrait. The vast majority of scientists perform honest labor and endeavor to use the scientific method as a guide to truth. The significance of fraud does not lie in its frequency but in the fact of its mere existence, and that it often goes undetected for prolonged periods. Fraud lies at one end of a spectrum of behavior that extends from deliberate falsification, to self-deception, to the necessary ignoring of anomalies when devising a theory. This spectrum of behavior is the dark side of science, the unacknowledged underworld of research. Yet it is just as important as the logical superstructure described by the philosophers and sociologists of science. Fraud is the key to the underworld, and to the understanding of science as a human process.

34

PART TWO

Three Sociological Perspectives on Deviance in Science

Part Two focuses on both the description and explanation of deviance in science. Chapter 3 by Kilbourne, Kilbourne, and Higgins places science within a particular societal context. Science is thought to reflect and to reporduce the society of which it is a part, and therefore scientific deviance can be said to be similar in kind and etiology to certain forms of societal deviance. In Chapter 4, Gaston argues instead that the unique structure of science minimizes scientific deviance, increases the likelihood of its detection, and, most likely, the magnitude of the problem is not as severe as some think. Meadows and Meadows offer a third sociological perspective in Chapter 5. While emphasizing the structural factors of scientific research, Chapter 5 specifically addresses the complexity of the problem by focusing on "companies of deviance," both internal and external sources of deviance, the researchers work situration, and the assessment of the consequences of deviance in science.

SCIENCE, DEVIANCE, AND SOCIETY

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In recent years there has developed a growing interest in the occurrence, range, and distribution of deviant behavior in the scientific community. This interest and concern has arisen in large part outside of the profession of science (primarily in the form of Congressional investigations [Marshall 1981a; Shapley 1977; Sun 1981a] and mass media coverage [Begley, Malamud & Hager 1982; Golden 1981; Hunt 1981]) while generally receiving minimal and oftentimes ambivalent attention from the profession itself (Broad 1981a). Sociologists concerned with deviance and crime have also generally neglected this area. However, it is evident that the increase in reported instances of deviance in science has become a focal concern for some (Culliton 1982; Sun 1982) and poses a pragmatic problem for the ideals and goals of science (Edsall 1981). Yet, to date there have been relatively few attempts to conceptualize systematically deviance in science or to understand why such behavior occurs.

Prior discussions of deviance in science have generally idealized science and have eschewed a more critical sociological analysis. A good example of this idealized and individualistic approach is evident in the work of Harriet Zuckerman (1977). Zuckerman suggests that deviant behavior in science is rare (i.e., limited to a few individuals and apparently only understandable in terms of personal motivations), and its incidence is largely explainable by the scientist's professional socialization and the unique norms of science. Such norms as "organized skepticism" and "the requirement of reproducibility" are purported to detect deviance within a reasonable time-frame and to minimize its occurrence. As a consequence, deviance in science is considered a unique phenomena and not adequately explained by contemporary theories of deviance (Zuckerman 1977).

Although Zuckerman provides us with a ground-breaking analysis of deviance in science, she nonetheless makes no attempt to address the societal context within which science and its norms are embedded, nor does she address the social historical forces that have contributed to the purported unique normative order of science. The problem with such an individualistic approach is that it tends to overlook the relationship between scientific deviance and particular vested interests (Adorno 1973; Habermas 1971, 1973; Hockheimer 1972), thinking styles (Berger & Luckman 1966; Mannheim 1936), social structures

(Merton 1939), and sub-cultural/cultural traditions (Sutherland & Cressey 1976).[1]

However, a more sociological approach to the study of deviance in science suggests we recognize that the *social institution* of science is situated within a particular societal and cultural context (Friedrichs 1970; Kuhn 1962; also see Lakatos & Musgrave 1970) and tends to reflect and even reproduce certain aspects of that context (Sampson 1981). Science is thus demystified and scientific deviance need no longer be considered an instance and confirmation of an isolated and individual act. To the contrary, scientific deviance can be henceforth examined in relation to other social structures, pressures and influences motivating people toward deviance in the larger society.

The purpose of the present paper is to proffer an alternative sociological conception of deviance in science. Five structural commonalities between science and society are identified and discussed as the basis for reconnecting deviance in science with deviance in society. These five commonalities are: (1) similar patterns of occupational deviance and white-collar crime; (2) similar opportunity structures and class divisions; (3) similar norms; (4) similar values; and 5) similar myths and myth functions.

Similar Forms of Deviance

A more sociological approach to the study of deviance in science can be derived from Richard Quinney's discussion (1964) of deviance in occupational and professional groups. Quinney has distinguished between deviance in such groups on the basis of occupational deviation and occupational or white collar crime. Occupational deviation consists of those forms of deviant behavior which are not within the purview of the legal system and which describe departures from the norms specific to a particular occupation or professional group. White-collar crime, on the other hand, is conceptualized as those behaviors within an occupation or profession that are punishable by law.

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[1] There are other problems with this perspective. For instance, while some have claimed that deviance in science is rare, that it is attributable to personal motivation by default since all social explanations have been ruled out, and that the same norms engendering intense competition in science also detect the occurrence of deviance (Zuckerman 1977; Gaston 1982, and printed herein), we should recognize the inherent limitation of this view, despite its obviously attractive appeal. Such a view is essentially a *closed system of analysis* which maintains itself by discounting *a priori* all alternative means of conceptualization and investigation but its own. The closed nature of the system is indicated by: (1) its dependence on support by argument in lieu of empirical evidence which is considered inadequate *ipso facto*; (2) the tendency to challenge the motivations of its detractors; (3) by claiming negative consequences may result for science if we investigate certain hypotheses (e.g., "the tip of the iceberg" hypothesis); and (4) a narrowed focus of inquiry to primarily instances of "manufactured data" at the expense of a larger appreciation of "companies of deviance" in science (Meadows & Meadows 1982, and printed herein).

The distinction made by Quinney between occupational deviation and white-collar crime can facilitate our understanding of the different kinds of deviance described elsewhere in science (Zuckerman 1977). For example, *occupational deviance in science* (i.e., the violation of norms specific to science) consists of *ad hominen* attacks, dogmatism, shoddy work, secrecy, milking data, and publicity seeking, etc. But there are other deviant activities in science which constitute criminal activity from the standpoint of the larger society. These latter activities consist of various forms of *white-collar crime*, and they are evident in various professions and occupational groups in society (e.g., fraud, theft, misappropriation of funds, discrimination, the restriction of competition, and the violation of procedural safeguards, etc.).

The concept of white-collar crime was originally introduced by Edwin Sutherland (1940) to explicate those acts of crime committed by persons of high respectability and social status in the course of their occupation or profession. Three central and distinguishing factors are associated with white-collar crime. They are: (1) the white-collar criminal emerges from a group which enjoys considerable economic, social, and/or political power; (2) the specific activities of white-collar criminals falling under legal sanctions and control may have long range and indirect consequences for society; and (3) the costs of white-collar crime to society has been estimated, in the long run, to be far greater than those costs associated with street crimes (Chamber of Commerce of the United States 1974; Conklin 1977). The higher costs of white-collar crime may be difficult, however, for some to appreciate due to the greater visibility, and oftentimes brutality, of many types of street crime. In fact, Sutherland (1940) argued:

> The financial loss from white-collar crime, great as it is, is less important than the damage to social relations. White-collar crimes violate trust and therefore create distrust which lowers social morale and produces social disorganization on a larger scale. Other crimes produce relatively little effect on social institutions or social organizations. (p. 5)

Interestingly, five correspondent or general categories of white-collar crime are evident in science and other occupational groups in society (i.e., corporate, government, union and professional). *First,* fraud is a form of white-collar crime and is indicated by: misrepresentation in advertising, selling adulterated goods, performing unnecessary operations, income tax evasion, violation of rent control ceilings, the issuance of phoney licenses, knowingly manufacturing defective goods and products, and misrepresenting repair needs and services, etc. *Fraud can also be considered a form of white-collar crime in the scientific community and is generally indicated in: forged and fabricated data, the exaggeration of findings, the suppression and/or indefensible manipulation of scientific data, and the performance of unnecessary and costly experiments.*

Examples of scientific fraud are evident in the following cases: (1) At Massachusetts General Hospital (Wade 1981), a researcher with $750,000 in federal funds forged his data, and for seven years watched over a cell line for the study of Hodgkin's disease that proved to be useless; (2) At Boston University (Broad 1981a), a scientist with nearly one million dollars for cancer research resigned when claims were made that his submitted reports contained repeated falsifications of data; (3) At Harvard Medical School (Broad 1982b), action was finally taken six months after a researcher admitted to falsifying results. Ostensibly, the deviant researcher was still involved in a $724,154 study funded by the National Institute of Health, even after allegations of fraud had been made (for further examples of fraud see Broad 1981b; Dorfman 1978; Gould 1981; Hixson 1976; Holden 1979; McKean 1981; Silberner 1982; Smith 1977a; Sun 1981c).

Second, theft as a form of white-collar crime in certain occupational groups in society has been generally conceptualized in terms of stealing, embezzlement and the use of public funds for personal gain and/or the padding of payrolls. In science, theft is most evident in the plagiarism of co-worker's ideas and findings, not giving credit to collaborators and other scientists, and in the sabotage of a rival's research. The extent of plagiarism in science and not giving credit to other researchers is suggested in a study by Warren Hagstrom (1974). In this study, 44% of the interviewed scientists alleged that other scientists had failed to acknowledge their earlier work, although they knew of it. Also, 12% of these scientists revealed having been accused themselves of failing to recognize the previous work of others. As Hagstrom notes, these controversies are not without consequence, for example, in appointing persons to university positions, granting formal awards, and in the competition for research funds. An example of this type of white-collar crime was recently revealed at Yale University (Broad 1980b) where a researcher who had considerable NIH support ($100,000 for 1980 alone) plagiarized a rival's paper and subsequently eleven of his papers were retracted (for additional examples see Goldhaber 1972; Hunt 1981; Sun 1981d). No less alarming than the above are recent allegations that even Nobel prize winners have sometimes failed to give proper credit to their early colleagues, collaborators, and/or co-workers (see Broad 1982c).

Third, misappropriation of funds constitutes another form of white-collar crime in American society. We can see this in those instances involving the pilfering and/or the diversion of investments and retirement funds, kickbacks, bribes, fee-splitting, and overcharging. In science, misappropriation generally consists of pilfering and diverting research funds, overcharging for providing scientific consultation, not releasing funds or academic awards for the purposes to which they were intended, and the pooling of grant monies in order to camouflage their manner of expenditure. The incidence and gravity of this type of white-collar crime in science has become an increasing concern for some Congressional committees (Shapley 1977). The activities under examination range from blatant misappropriation to the routine fudging of accounts. An example of this type of white-collar crime surfaced at the University of Colorado (Shapley 1978) when a researcher who had received more than 2.65 million dollars from the National Science Foundation, in his university capacity and as president of his private firm, was found guilty of falsifying his accounts. More recently, a pathologist (Broad 1980a) at the University of Wisconsin diverted $900 from his government grant to pay for ski trips (for similar examples see Broad 1982a; Smith 1979; Wade 1976), and NIH investigators (McDonald 1981) confirmed that an anthropologist at the University of Kansas "elicited kickbacks from research workers" and billed "unrelated costs" to an NIH grant (p. 16).

The *fourth* general category of white-collar crime has to do with discrimination and the restriction of competition. We can see instances of this category in those business and government settings involving price fixing, the violation of anti-trust laws, and/or discriminatory hiring/firing practices. *In science, we may also see the manifestation of this category of white-collar crime in terms of discriminatory hiring/firing practices and/or entrance requirements as well as in the restriction of competition and funding to only those scientists who satisfy certain arbitrary requirements.* An under-reported, but noteworthy, instance of discrimination in science is evident in a case involving allegations of political censorship of scientific publication. According to Altschuler, Moore, & Posner (1981), Claude Eon, former director of Technical Research for the Institute Mondial du Phosphate (IMPHOS), was fired when he opposed the arbitrary censorship of two Israeli papers from the published proceedings of their Second International Congress meetings.

Discrimination in science may furthermore extend to those cases where a scientific board has been established to select applicants for federal research awards and contracts and the screening committee selectively excludes certain kinds of research from funding, not because of the merits of the research *per se*, but rather due to ideological considerations or value conflicts. Certain forms of research such as the benefits of alternative lifestyles may be perceived as posing the possibility of destabilizing certain political forms and vested interest groups in government and society. Thus, exclusion and restrictive competition must be considered as forms of white-collar crime in the profession of science and in some science-related activities. This particular type of deviance gained public attention when NIH was charged in a lawsuit for discriminatory practices in the awarding of training grants (Shapley 1974). Also, in congressional investigations of NSF peer review policies (Walsh 1975), one frequent criticism has been that a few highly positioned people tend to misrepresent peer review comments.

Fifth, and lastly, there exists a general category of white-collar crime which relates to the violation of certain procedural safeguards and is most apparent in those cases involving the violation of specific labor laws. However, this correspondent form of white-collar crime in science appears less related to the actual conditions of employment of scientific professionals and more concerned with the conditions of recruitment and participation of individuals as subjects in scientific investigations. The violation of guidelines and laws to protect human and animal research subjects is conceptually analogous to the violation of labor laws to protect workers in society. Not surprisingly, such violations have become a major concern for federal authorities in recent years due to the increasing awareness by many of the attendant risks for individuals participating in certain kinds of scientific research.

According to Etizioni (1973), abuses in the use of human beings for experimentation are not uncommon. He cites the case of the Tuskegee syphilis study (see also Jones 1981) as one of the most glaring instances of such abuse. In this case, syphilis in poor black men was allowed to run its course so medical researchers could investigate the long term effects of the disease. Another case involved confirmed allegations by the National Institute of Health (McDonald 1981) that a researcher at the University of Kansas violated federal regulations governing the protection of human subjects. Apparently the researcher in question had taken unauthorized blood samples from more than 500 children in Beize (formerly British Honduras) and used untried methods of genetic screening without proper clinical support. And at UCLA (Sun 1981b) NIH stripped a researcher of grants totaling $190,000 because he prematurely and illegally conducted the first gene therapy experiments on humans (for additional examples see Holden 1976; Kehrer 1982; Marshall 1981; Powell 1981; Restak 1973; Silberner 1982; Smith 1977b; Szulc 1977).

Although the costs of the above examples of white collar crime in science and society would seem obvious, Stotland and Geis (1980) contend that most white collar crimes are not reported at all or not reported in ways amenable to the collection of data. Similarly, with respect to the scientific professions, the general absence of systematic reports (Zuckerman 1977) of deviant behavior is acknowledged, but without giving any convincing rationale for this absence. We are consequently limited in what we can say about the *extent and incidence* of occupational deviance and white-collar crime in science. Notwithstanding, a number of factors have obviously contributed to the absence of its reporting. These are: (1) *denial*--on face value alone, the ideals of science tend to inveigh against the presumption of deviance, and the goodness or attractiveness of a reputation often provides the palmbearer with immunity from scrutiny (Broad 1981a); (2) *lack of sophistication on the part of social control agents and the public*--many of the activities and procedures of science are not generally understood by the lay public. Traditional methods of detection are completely inappropriate for uncovering many of these types of deviant and criminal behavior (Parker 1980); (3) *suppression*--because of the dependency of the scientific community upon outside sources of funding, there is always the potential motivation to cover up any form of scientific deviance. The high-risk plight of whistle-blowers (Edsall 1981) encourages such suppression since often the sanctions for exposing deviance in science are more severe than for those engaging in actual deviance; and (4) *perpetuates the status quo*--certain forms of deviance in science (e.g., fraudulent data that perpetuates the dominance or position of particular vested interest groups [males, whites, upper income groups, etc.]) may be consistent with the status quo or power elites and therefore rationalized or not even considered deviant.

In summary, when we use Richard Quinney's (1964) conceptual distinction between occupational deviance and white-collar crime, deviance in science becomes remarkably similar to deviance in other occupational and professional groups in society. In essence, prior individualized discussions of scientific deviance and idealizations of the scientific community have tended to obfuscate this conceptual similarity. They have also tended to render scientific deviance theoretically unrecognizable (see Zuckerman 1977). Such arguments generally relegate scientific deviance to personal motivations and allege its occurrence is mitigated by the unique structure of science (Merton 1973).

But of course, from the present sociological standpoint, certain forms of deviance in science generally resemble white-collar crime in other high status professional groups in our society. There is an overwhelming tendency to rationalize it, ignore it, and to suppress discussion of it. Those engaging in such criminal activities are usually not apprehended, rarely prosecuted, and typically encounter no more than a hand slapping or symbolic punishment. What remains unanswered, however, is the degree to which the costs of white-collar crime in science reflect the high costs of white-collar crime to society, even if the proportion and incidence of its occurrence is shown to be minimal or less extensive.

Class Divisions: Society and Science

Another way the structure of science and the structure of society are similar is in relation to their class structure or class divisions. The stratification of American society (U.S. Department of Commerce 1980) and the differential availability of life conditions has been well documented over the years. Individuals in American society generally have a better chance for material success, job satisfaction, security, health, and high status, etc., if they are simply born into an upper strata family than if they have to depend on the availability of equalizing opportunities (Caplovitz 1979; Gortmaker 1979; Olneck & Crouse 1978; Sewell 1971; Wilhelm 1979). Similarly, the class divisions of science tend to reflect the class divisions and the differential availability of opportunities in American society. This is indicated by the following: (1) sex discrimination exists within science and has been found to adversely affect women's professional outcomes (Astin 1969; Kashket, Robbins, Leive & Huang 1974; Lewin & Duchan 1971; Marshall & Perrucci 1981; Reskin 1976; Simon, Clark & Galway 1967); (2) women still have higher unemployment rates and lower salaries in the scientific professions and the percentage of non-whites in science is lower today than 10 years ago (Bromley 1982; Vetter 1981); (3) association with high productive or high status scientists increases the likelihood of being brought into a larger network of influence and communication and having greater access to rewards within the scientific community (Crane 1969); (4) there exists a greater and disproportionate likelihood of continued recognition of persons already prestigious in science and the subsequent withholding of recognition from researchers who do not enjoy high prestige (Allison & Steward 1974); (5) there is a pronounced tendency for editors of social scientific journals to be differentially selected from certain universities (Yoels 1974); (6) scientists trained at less prestigious universities tend to have greater difficulty in developing their research activities, acquiring advantageous research positions, and have less chance for recognition (Caplow &

McGee 1958; Crane 1965, 1970); (7) scientists tend to gain more from affiliation with a major university than from high productivity (Crane 1965); and (8) recruitment into major universities operates in some cases independently of productivity (Long 1978; Long & McGinnis 1981).

In the above sense, citizens within American society and scientists within the scientific professions share a common problem of differential access to their respective opportunity and reward structures. The class divisions of science and society are remarkably similar, and the differential availability of opportunities in each tends to affect one's likelihood for success and recognition. Individuals in science and society should consequently experience similar structural strains towards social deviance.

Similar Norms

The structure of science also resembles the structure of society in relation to their similar normative orders. According to Robert Merton (1973), there exists within science two classes of norms (i.e., cognitive and moral) that are intertwined in practice and directed toward the implementation of the goal of scientific activity -- "the extension of certified knowledge about the natural world" (Zuckerman 1977:87). Cognitive norms specify what should be studied and how, and moral norms express the "proscriptions, preferences and permissions concerning the attitudes and behavior of scientists" (Zuckerman 1977:87). Departures from the cognitive and moral norms of science have been viewed by Zuckerman and others (see Gaston 1978) as forms of deviant behavior.

Although Mulkay (1976) contends the specific content of moral norms (i.e., disinterestedness, rationality, etc.) may extend beyond the scientific community and permeate (at least on an abstract level) other groups in society, there has been little appreciation of the similar bifurcation of norms (i.e., cognitive vs. moral) in science and society. *It is as if the norms of science have cryptically emerged independent of other influences in society*. Such an implication tends to obfuscate the common social influences pervading throughout the profession of science and other professional groups in contemporary society.

In a general sense, the moral and cognitive norms of science are directly analogous to what Deutsch and Gerard (1955) refer to as the normative and informational social influences of the group upon individual judgements. Normative social influences pertain to the individual's desire to create and maintain favorable relations with others (i.e., preferences and permissions concerning a person's attitudes and behaviors, etc.) and have been experimentally demonstrated in the Sherif study (1936). Informational social influences refer to the tendency for individuals to use the judgements of others as information about what constitutes reality (i.e., what is worthy of consideration, why, when, and how, etc.) and have been experimentally demonstrated in the Asch study (1956). Both normative and informational social influences are widespread in contemporary society and tend to manifest themselves in diverse ways.

When we take into consideration the work by Deutsch and Gerard (1955) and its general applicability, it becomes extremely difficult to distinguish conceptually normative and informational social influences from the moral and cognitive norms of science. We are led into the conceptual corner of being compelled to acknowledge the overt similarity between the normative orders of science and society. Such a recognition becomes even more compelling when we consider how norms develop. Norms typically develop to legitimate the values and interests of high status and high power members of the group or community (Backman 1979). This suggests that over and above the similar normative conditions in science and society, there may be common values and interests served by such normative conditions.

The normative order of science appears, then, to legitimate in many respects the normative order of society, and the former tends to perpetuate both the latter's continuance and those high status individuals in society who benefit from it. Individual members in society can point to the norms of science to justify and rationalize certain prevailing norms (e.g., rationality, objectivity, etc.) in society. Similarly, in some religiously oriented societies and in some religions (Wallace 1966), people can point to the similar stratifications in heaven to justify extant stratifications in society. The similar norms in science and society also suggests there should be important similarities in the kinds of departures from such norms and in the pressures inductive of deviance.

Similar Values

The structure of science tends to reflect the structure of society in relation to their shared underlying values. This is not surprising given the similar norms in science and society and our knowledge of how norms shape behavior in the direction of shared values (Secord & Backman 1974). Indeed, Sampson (1976) has commented upon the direct connection between the dominant scientific paradigm and the central value orientation in society. According to Sampson, the dominant naturalistic conception of science--which searches for laws of nature independent of the socio-historical context--has itself emerged within a particular socio-historical context (Kuhn 1962) and emphasizes, like the society in which it is embedded, the values of Protestantism, liberalism, individualism, male dominance, and marketplace thinking. The employment of one particular paradigm, as opposed to another, is in large part a political decision and tends to reflect, and to resurrect, a particular cultural ideology and value orientation.

For instance, out of the Middle Ages emerged a total social context appealing to the spirit of liberated individualism, where both Protestantism and science constituted efforts to break free from earlier and oppressive social forms (Merton 1968). This parallel thrust of the ethos of religion and the contemporary paradigm of science (Sampson 1976) is indicated in their shared emphasis upon skepticism, cognition, the material world, positivism, individualism, and democratization/universalism. The natural paradigm of science has evolved, in turn, in such a way as to contribute to the development and maintenance of the existing social order (i.e., a stratified and industrialized society organized on the basis of marketplace values). Science and technology have been used to confirm through differential access, production and distribution an equity view of the world (i.e., differential rewards on the basis of differential inputs) by insuring that the differential availability of knowledge and technology mirrors the stratification of citizens in society.

The dominant naturalistic conception of science has also perpetuated the belief in the greater value of maleness (Carlson 1972; Bernard 1978; Rossides 1978; Eagley 1978) and the inevitability of male dominance. We can see this clearly in the almost total dominance by men, their theories, and their empirical investigations in the everyday practices of the natural paradigm of science. We can additionally see this on the symbolic level in the general tendency to reject alternative conceptions and models of nature and society which contravene prevailing assumptions and male-oriented interests.

Science furthermore tends to perpetuate the worshipping of the individual by attributing significant scientific advancements to "great men" instead of to the evolution of the discipline. Individuals are thought to account for the evolution of the scientific community and not vice versa. The dominant conception of science thus confirms the individualistic world view arising out of our economic, religious, and social traditions and tends to reify values characteristic of our culture.

In sum, a similar value constellation of empiricism, individualism, capitalism, Protestantism, liberalism, maleness and instrumental-thinking is evident in science and society. Such a value constellation in science tends to serve a reproductive and rationalizing function in relation to those same values in society. Hence, deviance from the similar norms of science and society may be related to the conflict of some with the common underlying values in science and society which reflect both our cultural heritage and the vested interests of high status members of society.

Similar Myths & Myth Functions

Many of the myths of science are also remarkably similar to the myths of society. This appears to have resulted from the increasing assumption by the scientific community of certain ritualistic practices (i.e., technology, therapy, social control, salvation and revitalization) which were previously enacted by other institutions in society. The enactment of these ritualistic practices by the scientific community has stimulated the emergence of correspondent myths in science and society. As Wallace points out (1966), ritual functions to induce the creation of myths, and myths function to rationalize the particular needs of the culture. The correspondent myths of science and society similarly rationalize the needs of our culture and generally pertain to our efforts to justify the way we have organized society around the imperatives of capital accumulation, growth, social control and differential attainment.

Five correspondent myths are evident in science and society. They are: (1) the myth of the invisible hand in society and the myth of self-correction in science; (2) the Horatio Alger myth in society and the myth of universalism in science; (3) the myth of "manifest destiny" in society and the myth of accumulated knowledge in science; (4) the myth that justice is blind in society and the myth of objectivity in science; and (5) similar origin or "great men" myths in science and society.

First, the myth of self-correction in science rests upon the belief in the "organized skepticism" and the self-policing nature of science. The basic idea, which is analogous to Adam Smith's idea of an "invisible hand," is that science, if left alone, will correct any distortions or malfunctions in the same automatic manner that the open market, if left alone, will balance the economic forces of supply and demand in order to insure competitive prices, maximum production, and full employment in society.

Second, the myth of universalism in science is directly analogous to the Horatio Alger myth in American society. Scientific contributions are purportedly assessed on the basis of certain agreed upon cognitive and public criteria and are not affected by the factors of class, race, gender, academic background or access to social networks. Hardwork and talent will allow anyone to pursue the advancement of knowledge and to achieve recognition by virtue of one's contributions. Thus, all have the opportunity to pull themselves up by their bootstraps and to get ahead either financially or scientifically.

Third, the belief in the progressive accumulation of knowledge in science is not unlike the myth of "manifest destiny" that we, as Americans, have been socialized historically to believe in. The traditional view of science is that science is accumulative and progresses by empirically disproving theories (Popper 1959). Continued research extends our understandings and propels us into an ever increasing body of scientific knowledge and wisdom. New frontiers are forever on the horizon as we uncover the facts and laws of nature. How similar this sounds like the myth of "manifest destiny" which, beginning perhaps with the desire and fervor of exploring the Americas and settling the West, has led us into the exploration of our deepest oceans and into outer space. Thus, both in science and society, each and every encounter with the unknown leads us one step further toward our destinies of discovery, conquest and mastery and implies we are a special people with a unique mission for humankind.

Fourth, there are the correspondent myths of objectivity in science and blind justice in society. A test of objectivity (i.e., a scientific investigation) before the eyes of the dispassioned and disinterested scientist is conceptually analogous to dispassioned objectivity before the eyes of the law. Objectivity tests, in both instances, presume truth is value-free and above ideological considerations. And for the impatient amongst us, we often hear that both the wheels of truth and the wheels of justice move slowly. Unfortunately, values often enter into the picture to bias to varying degrees the socially constructed verdict or fact. The objective is infused with the subjective. Our society has the need to rationalize the purported equity of the legal system for all members of society, and science has the need to rationalize the truth value of various findings and theories in order to maintain a sense of equity amongst all members of the scientific community.

Fifth, there is the marked tendency for us to celebrate heroes in science who have slain the dragon of metaphysics (Samelson 1974) and to celebrate heroes in society who have contributed to the democratic forms of our present way of life. We often search back into the historical past to identify those persons resembling the orientation or perspective of a particular contemporary discipline, philosophy or way of life. The creation of origin myths of this kind serves to justify and to perpetuate the 'rightness' and longstanding nature of a particular world view (e.g., the value of individualism) and certain structural arrangements in society and science. Origin myths in science and society serve a social control function by psychologizing success, legitimating the status quo and by confirming the longstanding truth value of particular viewpoints.

In sum, similar myths and myth functions are evident in science and society. This shared myth structure serves to rationalize existing social structures and social arrangements and tends to insure their mutual perpetuation, as well as to confirm the 'goodness' of extant social relations in society which are also evident in science. Consequently, some deviant acts within science might be attributed to the breakdown in certain myths, the correlated loss of social cohesion and/or to challenges to particular vested interests in science which are served by such myths.

SUMMARY AND CONCLUSION

The present paper has proffered an alternative sociological conception of deviance in science. Scientific deviance was described in terms of occupational deviance and white-collar crime and was explained in terms of some of the same social structures and social pressures motivating individuals toward deviance in the larger society (e.g., similar class divisions, norms, values and myths). This analysis rests on the assumption that the structure of the social institution of science cannot be understood apart from the structure of society and provides us with the starting point for a critical analysis of deviance in science.

When science is situated in a particular societal context and when the structural commonalities of science and society are appreciated, scientific deviance need no longer be considered a cryptic and mysterious phenomena. Rather, it can be viewed within the parameters of societal deviance and other related activities in society. We need not presume an indefensible belief in the unique structure of science to discount and/or to minimize scientific deviance. The similar structures of science and society as well as the interface of science with other institutions in society (Bromley 1982) would seem to preclude such a belief. It is almost inconceivable to imagine that science could exist in a socio-historical vacuum or to consider scientists could rub shoulders with people in other segments of society, on an everyday basis, and still be unaffected by such interdependent exposure. Quite the opposite, scientific deviance is very similar to certain forms of deviance in the larger society (i.e., occupational deviance) and necessarily violates a presumption of honesty in the quest for truth (Begley 1982). This results in making scientists more like their counterparts in the other professions of law, business and government. *The structure of science tends to reflect and to reproduce the structure of society (Sampson 1981), and consequently, scientists experience many of the same social pressures, aspirations and financial burdens that are common to other members of society.*

Moreover, there appears to be little evidence of the effectiveness of "organized skepticism" to minimize deviant activities in science in light of the growing size of the research enterprise and the general reluctance of scientists to perform systematic replications. Indeed, there is some evidence which suggests that independent checks (e.g., auditing, reviews, sales reports and impact studies) in both industry and government are more frequent and perhaps more effective than the almost dogmatic over-reliance upon non-systematic and infrequent replications in science. At this juncture in the history of science, the view that "organized skepticism" will quickly and inevitably detect deviance appears to be no more than an ideal or myth in the everyday workings of scientists.

The present paper, however, has not discussed various forms of *theoretical and methodological innovations* which from a broad perspective could also be considered as forms of deviance in science. Nevertheless, the present conceptual dichotomy of deviance in science suggests that such innovations should be considered apart from occupational deviance and white-collar crime in that they tend to support the ideals of science and not to inveigh against them.

A distinct advantage of the conceptual approach of the present paper is evident in relation to the general relevance of deviance theories to a discussion of deviance in science. Although some believe (Zuckerman 1977) that current theories of deviant behavior are limited when applied to scientific deviance, the present approach suggests these theories are more relevant than previously believed. Those conceptual variables found related to deviance in the larger society (e.g., differential opportunities, differential association, deterrence, conflict, labeling, and anomie, etc.) can be related to deviance in science, without lightly dismissing their importance and relevance. A brief review of Merton's (1939) anomie theory, as a case in point, will make this contention more apparent.

Anomie theory is pertinent to a discussion of scientific deviance for the following reasons: (1) *the class structure of science*--given the extant class divisions and differential opportunities in science there should subsequently arise anomic conditions which are similar to those same conditions in the larger society; (2) *diffuse anomie*--a condition which generally results when the cultural emphasis shifts from the satisfactions derived from the competition itself to an exclusive concern with the goals. Both Hagstrom (1974) and Broad (1981a) claim there is increasing evidence of a new emphasis in the scientific community upon the sought after goals; (3) *shifting definition of success*--evasive goals signal inaccessibility to all members of the community. The extreme emphasis upon recognition in the scientific community tends to create relative and evasive goals; and (4) *Merton's typology* (i.e., innovation, ritualism, retreatism, and rebellion)--can be used to incorporate instances of both occupational deviance and white-collar crime.

In conclusion, there are certain prevailing forces in contemporary science and society (e.g., excessive competition, differential opportunities, careerism, rising expectations, value conflicts and vested interests, etc.) which may lead some individuals in science to engage in scientific deviance and/or to rationalize such activities. Scientific deviance is an example of a *rationalized activity* in science that is directly analogous to certain rationalized activities in other sectors of contemporary society (i.e., white-collar crime in other professional and occupational groups). Individualistic and idealized explanations of deviance in science function in effect as self-serving professional ideologies and only tend to obfuscate the problem.

Similar to other professional groups in society, scientists have a vested interest in maintaining a favorable image of themselves and of what they do. Activities related to image-maintenance help to insure the professional standing of the scientific community and the subsequent goodwill of society toward the science industry. Not only do professionals and organizations develop *impression management strategies* to enhance their public image, but they also develop certain assumptions about their behavior and the behavior of others in order to rationalize their professional activities (Goffman 1970).

Finally, to the extent we ignore the structural commonalities of science and society in general and how deviance in science is related to deviance in society in particular, we may be inadvertently contributing to the deployment of science as a social control mechanism. Certain kinds of science may be suppressed or not funded in order to ideologically justify the perpetuation of certain social forms and social organizations in science and society. In contradistinction, our ideal is that the purpose of science is to generate accurate explanations and understandings about ourselves and the world around us (Toulmin 1961) and, in so doing, should not favor certain individuals and groups at the expense of others. In this way science can serve a liberating role in the lives of all people and not serve a social control role for particular interests in a pluralistic society.

ACKNOWLEDGEMENTS

An earlier version of this paper was presented at the Sixty-Second Annual Meeting of the Pacific Division, American Association for the Advancement of Science, University of Oregon, Eugene, June, 1981.

We are especially indebted to the helpful comments and suggestions made by Paul Meadows, Peter Meadows, James T. Richardson, and Edward Sampson.

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ANOTHER SOCIOLOGICAL PERSPECTIVE ON DEVIANCE IN SCIENCE

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Deviant behavior is fascinating, especially that of others. Pick up a newspaper (and certainly some newspapers), turn on any radio or TV news program (not to mention the serial programming), examine almost any novel, view movies and films, and survey conversations at cocktail parties and other occasions where people meet and converse. If you do, you will rediscover what you already knew: the favorite theme is people doing things that some others think they ought not to be doing. As far back in history as we can imagine, the deviant exploits of people have saturated the public's attention. Lust, corruption, and perversion are common themes throughout literature, history, and art, and there is little reason to believe that such interests will decline.

Who would not rather read a story about a public figure's private life, complete with all the intimate details, than to read about the same person's political and legislative accomplishments. The good works of people may not go unnoticed or unpraised, but when people do what is expected, it is routine. Doing the unusual or what people are not supposed to do is what is exciting.

Deviant behavior of scientists, too, is much more interesting and fascinating than conforming behavior. We know well how scientists are supposed to act. But our goal here is not to discuss the causes and consequences of that usual mode of behavior, that routine and overwhelming volume of human action. Instead, the purpose is to examine the "darker side" of science.

My attempt to put a particular perspective on deviance should not cause the reader to infer that the act of deviance is unimportant or unworthy of scrutiny because I certainly believe that deviance in science is a problem. The point of this paper is not to offer an apology or to defend science or scientists unequivocally. The three points I shall present include: (1) the magnitude of the problem of deviance in science may not be and probably is not so severe as some apparently would have us believe; (2) the reason why discovered deviance is probably infrequent is a consequence of the structure of science; and (3) when deviance is made public, processes in science produce a consequence for the guilty potentially more severe than in other institutions in American society.

These points, which attempt neither to cover up nor to minimize the seriousness of deviance, are intended only to put the problem into context. I will mention at the conclusion some recommendations which will address the problem directly, and I think enhance the probability that science will not suffer severe repercussions of withdrawn moral and financial support from our society because of future disclosures of deviant behavior.

Deviance, Structure, and Justice in Science

The incidence and prevalence of deviance in science are at undesirable levels, not because of how frequent and how much there is, but because there is any at all. Remember, however, that deviance extends over a wide range of behavior. On the one hand, and the least extreme, scientists either intentionally or through oversight, fail to refer to work that helped or influenced them in their research. No agreement exists, of course, on a specific incident, either to the fact or its motivation. Whatever the reason for such failure, their deviance is available for everyone to see. As the saying goes, whereas physicians may be able to bury their mistakes, scientists' mistakes are indelibly printed and distributed for contemporaries to see. Moreover, they are stored in archives so that in the future any number of people can have an opportunity to see them.

Somewhat more severe among deviant acts is presenting results prematurely or withholding information too long. Premature disclosure is difficult to fault because it conforms to the norm of sharing results. (For a discussion of some norms of science, including this one of communality, see Merton 1957:550-561.) But the usual assumption is that scientists present prematurely to establish priority. While holding on to information too long tends to violate the norm of communality, the usual assumption is that enhancing validity is the reason for the delay, not establishing priority. Both of these acts can have consequences for others in causing them to waste time and resources because of being misled or uninformed.

On the other hand, at the extreme end of the deviance continuum, some scientists do engage in severe types of deviant behavior. They cheat in their procedures and lie about their results. This deviant behavior naturally has different but severe consequences both for science and individual scientists.

Deviance: Increasing, Decreasing, or Stable

In the last few years, two types of information have caused me to think that many scholars perceive deviance in science is increasing. First, there are the undocumentable conversations with some scholars at meetings and symposia (including the one in which an earlier version of this paper was summarized) who suggest that recent exposed cases indicate increased deviance. They argue that the blatant cases publicized in various news media show that deviance is becoming a more frequent act and also that surely much more deviance is undetected. But there are documentable cases as well. For example, one passage in a paper distributed originally at this AAAS symposium (Meadows & Meadows 1982 and printed herein) states when referring to the varied reports on science:

> But from another group, increasing in number, it is being reported as something different from our traditional picture of the House of Science. They report a growing pattern of science research behaviors which never seem to have belonged to the Old House: cheating, data concoc-

> tion, fact fudging, lying, fraud, among other malfeasances, which we are still hearing about.

A second source, unfortunately, is unpublished papers that I have been asked to referee anonymously, and which, as far as I can discover, have not yet been published. Most of these have taken the position, using the handful of cases publicly described, that these cases show a significant increase in the incidence of fraud. This seems to be a rather ahistorical view because usually no attempt is made to compare these incidents with earlier ones in the history of science.

A third source is the news media. In the last few years, Science has published stories in which the exploits of scientists accused of deviant behavior have been described (for a sampling, but by no means exhaustive list, see Broad 1980a, 1980b, 1980c, 1980d, 1980e). This has probably helped focus attention and discussion, but may have provided perceptions that cannot be validated by current data. For although careful reading will not find explicit conclusions or predictions that fraud is increasing, the tone of some of the papers seems hardly designed to convey the opposite. (I would settle for withholding judgement until sufficient data are in hand.) For example, in the description of Phillip Handler's response to Congressional inquiries about fraud in science, the story itself reported this: "Phillip Handler, president of the National Academy of Sciences, put it more bluntly, saying that the fraud issue is 'grossly exaggerated' and should be handled internally by scientists" (Broad 1981a:421). In the title of the story, or what might better be described as a headline, we find "Congress Told Fraud Issue Exaggerated" which can easily be inferred as mocking the notion that the issue is exaggerated at all. Of course, editors may have more influence than reporters over the titles of such stories--I cannot attribute authorship to the title.

Another instance is a story, although a complicated example, that can be best appreciated only if read in its entirety with all the nuances. Its title, "Toying with the Truth to Win a Nobel," has the further brief "abstract" of sorts which says, "Amid a priority dispute, J.J.R. Macleod in 1922 disclaimed credit for the discovery of insulin; 2 months later he told a different story to a Nobel emissary and thereby won the call to Stockholm" (Broad 1982:1120). The "abstract" seems to be hardly more than a "grabber," because after reading what is a discussion of a new book on the topic one can reasonably conclude that the book being discussed does not agree with the story title (headline) or abstract. This, of course, may tell us more about the reporting of deviance in science than about deviance in science.

Only a few systematic attempts to examine the prevalence of deviance in science are available. Zuckerman (1977) has surveyed these reports and concludes that the evidence is problematic. Only a few ways exist to discover the prevalence (amount) or incidence (rate) of any kind of deviant behavior. Generally, official statistics are the most comprehensive, but they are notoriously incomplete, and, in any case, none exist for deviance in science. Self-reports are one method, and I have used that approach for less serious types of deviance (Gaston 1973). In that study, the original interest came from Hagstrom's (1965, 1967, 1974) work about potential areas of secrecy. At first, I expected secrecy would be prevalent among British high energy physicists. The data showed, however, that much of what Hagstrom may have uncovered about secrecy actually may not have been a violation of the norm of communality. Instead, depending on the stage of research at which a scientist may exhibit secretive behavior, scientists' willingness to discuss their work is a reasonable caution against misleading others. Ben-David (1977:264-265) elaborates the appropriateness of this type of behavior and how it conforms to the norms of science. But although some scientists may reveal cases of personal deviance, others may not. And it is reasonable to believe that if we tried to get scientists, in some routine fashion, to admit to the most serious act of fraud--suppressing, fudging, or fabricating data (see Zuckerman 1977:113)--we might not have much success, and certainly we would have data of unknown accuracy.

If "official" data are unreliable and unavailable in any case, and if self-reports are difficult to obtain, a third strategy--obtaining data on victims--has proved to be an excellent method in the larger society for instances of theft and assault. However, scientists may be, and I think would be, reluctant to accuse someone privately of fraud in an interview. This reluctance is not the same kind of response as a citizen's reluctance to "become involved" as a witness in a robbery or murder case. Instead, to accuse someone of research fraud--unless one is a first-hand observer--is risky, for it is clearly the case that suspecting a set of results may be "too good" is insufficient to justify a suspicion of fraud because too many other explanations may be valid.

Some types of deviance in science which are potentially serious for scientists as victims, such as having ideas stolen or an even less serious type that of not having one's work cited, can be a form of victimization more apparent than real. Thus, although it is not unimportant to know the extent and distribution of such beliefs by scientists about their being victimized, the testimony of individual scientists can hardly be accepted in an aggregate fashion as valid indicators of the prevalence of other's deviant acts. Indeed, the number of times that any of us are likely to believe we ought to have been cited or to have arrived simultaneously at similar ideas is tremendous. Collecting such "victimization" data to assess the true prevalence or incidence of deviant behavior would certainly be difficult (because some cross-checking criteria would be necessary), if not impossible to achieve. This leaves us with the final strategy of first-hand observation of deviance, but few would expect that method to be viable for obtaining defensible data.

Even if we were confident about some systematic data from the contemporary period, and had equivalent data on historical periods, the current size of the scientific community would require a very high number of cases to equal the rate of even a few cases historically. And, I should caution that the number of research years at risk, rather than the number of individuals, would provide the appropriate basis for comparing rates, and that could increase dramatically the comparisons because the average life expectancies have increased considerably over the last forty years. Thus, I would be surprised if historical periods did not produce a larger rate of deviancy--however measured--than the post-World War II period.

Notwithstanding, the absence of appropriate data cannot substantiate my argument that the amount of deviance today may be no greater than it ever was. Yet, if the absence of appropriate data cannot substantiate my thesis, neither can the lack of data substantiate the opposite thesis. Hence, I would argue for extreme caution in drawing conclusions about the amount of deviance in science.

It is not the extent to which any of these deviant behaviors are publicized that surprises me. What is problematic are some of the conclusions and generalizations which are presently being publicized about these few known events. For if there is one thing that scholars should be able to agree on--but probably do not--it is that we can have no confidence in the conclusion that such behavior is increasing or that such behavior which is discovered is sufficient evidence to allege it is only the tip of the iceberg.

I am aware my position is not in the mainstream of much of the current writing. I am also aware my point may be criticized as simply a conservative argument in favor of the status quo in science. However, whatever the ultimate validity of the proposition that science does not contain as much deviant behavior as some seem to believe, my advancing that position does not have potentially negative consequences for science. In contrast, writers who imply or suggest that deviance in science is on the "upswing," or that there is a "rising tide," or similar notions of a spreading epidemic, can cause negative consequences for science. Additionally, while it is possible to attribute my position to an ideological base--that my motivation is to "defend" science because of a vested interest in the status quo--a similar conclusion could just as easily be advanced for those making such an attribution. Namely, ideological motivations may exist among those who conclude there is an increase in scientific deviance. It is certainly reasonable and logical to ask that if such dramatic conclusions of the "increasing rate" variety are based on incomplete and insufficient data, why then are they stated and repeated with such conviction?

Certainly, I have no data to answer this question, although I would suggest that any study of scientific deviance ought to include an assessment of the level of hostility and envy toward science present among some scholars in the research community. Hostility and envy are emotions not unknown among scholars as among other people.

At the same time that one frequently hears descriptions of the alleged increase in deviance, the next thing one hears about are the dire consequences this is going to have for the moral and financial support of science by the American public. We should not dismiss the possibility that those who argue, in the absence of sufficient data, that deviance in science is increasing are also wrong (so far, at least) about the purported "loss of confidence" in science that recent news of deviance has produced (see Walsh 1982:270-272; National Science Board 1977). In light of the evidence from national surveys, notions about the news of deviance and the consequent decline in public support must be questioned. Why is there a persistence in such predictions? One can only speculate.

The Structure of Science

Leonard and Weber (1977) call sociologists to task in the study of white collar crime because "Insufficient attention has been focused by sociologists on the extent to which market structure--that is, the economic power available to certain corporations in concentrated industries--may generate criminal conduct" (1977: 134). That observation is pertinent for two reasons. First, it suggests that we need not fear the possibility that universities will come to resemble an oligarchy and become witting or unwitting harbors for wayward scientists. Second, and to the main point, it suggests that deviant scientists are part of a social structure which produces their behavior, and at the same time, paradoxically, increases the likelihood of their detection.

The structure in which scientists operate may be described in this way. Scientists are motivated to do original work and to publish their results in order to gain recognition. The most important work will receive the most recognition. As in classical economic capitalism, competition is ordinarily keen. The reward system distributes recognition according to the importance of the contribution for the enhancement of science (of course, there are many smaller systems at the disciplinary and specialty areas working inside the scientific community). Recognition is the coinage of that realm and scientists without it are relatively poor; scientists with it are relatively advantaged. At the same time, some scientists have high visibility and high prestige, and others have low or little visibility and consequently little prestige.

Motivation to gain recognition is a strong force influencing scientists, and some individuals are going to cut corners and deviate from the expected and usual behavior. But at the same time, scientists are located in a structure that also minimizes deviance. The following reasoning allows us to appreciate "why" if the rewards of being recognized are sufficiently attractive to risk exposure of their deviant behavior, then the research problem is important enough for others also to be working on the same or similar problems. And if one person or group is not the first to make a discovery, they will try to replicate or reproduce the important results found by others.

One aspect of science that distinguishes it greatly from other social institutions in society is that every member of the scientific community is potentially a policeman. Individuals who cheat the IRS may brag about it. Spouses who cheat may tell friends. Physicians who make mistakes may tell a loyal confidant. And when such disclosures occur, the guilty are rarely called to task by those informed. Instead, it is usually others in the formal social control system who do the detecting. In science, however, it is one's colleagues--locally, nationally, or internationally--who first suspect and then act on their suspicion. And the deviant scientist's results, if important enough to warrant the scrutiny in the first place, are likely to have been published so that little opportunity exists for claiming, "I did not say that!"

If, however, the problem is not of much importance, and the rewards commensurately less, then the risk of exposure is small. This might apply to scientists located at institutions where the level of research is not close to the frontier, and although the potential rewards for results produced by deviant acts might be substantial for the deviant individual in that parochial location, the rewards from the scientific community are not going to accrue.

The social structure of science does not guarantee absolute conformity to the norms of science, and it may be that the deviance we see is only the tip of the iceberg. But if that is true, it is what we see that is important.

Justice in Science

The processes which operate in the social structure of science probably produce the most unusual relationship between the guilty and the penalized. The higher the position, the more likely the detection and the fact of detection virtually carries an automatic penalty. This may seem strange because of the demonstrable different locations of power and prestige that scientists occupy in the social stratification of science. Generally, one's position in the social structure produces many positive or negative consequences, depending on being high or low respectively. In the prestige system of science, however, one's position results in some unusual features.

Even if the predicted low prevalence of deviance in science eventually is shown to be incorrect, the unusual feature about deviant individuals in science is that they should not escape the "system of justice" because of their preferred status, although certain individuals in high status positions in society frequently do. There is little need to list the politicians in high places whose deviant acts have resulted in lenient penalties. Similarly, there is considerable evidence that executives in business and other organizations rarely suffer long prison terms, if at all, for their skills at embezzling. At the less advantaged level, in contrast, lower class individuals tend to serve longer sentences than upper class individuals, and blacks apparently receive more severe penalties than whites. It is not only the lesser severity of punishment that favors prominent individuals, it is also the belief, though somewhat difficult to substantiate conclusively, that such individuals disproportionately escape detection altogether, or that if detected, they are not convicted (a condition that is less difficult to substantiate).

But how do detection and sanctions for deviance in science compare to that in other areas? The general pattern is, as I indicated earlier: detection of deviance is much more likely for visible and prestigious scientists, and if discovered, the sanctions are more serious. That pattern is the opposite from deviance in other institutions in society.

Consider detection first. At the high levels of the social stratification system in science, the scientists who have earned those positions are more likely to have made and to continue to make further significant discoveries. The more important the discovery, the more likely the norm of organized skepticism will be in operation (see Merton 1973), and the more likely that others will attempt to reproduce the discovered processes in order to confirm the results. The more attempts to confirm, the greater the chances are to discover error or fraud, if it exists. Although the norm of organized skepticism should work equally to accomplish the same thing for scientists at the lower levels of the stratification system, activity and resources in the scientific community must be focused on replication for important, not routine work. Therefore, the more routine and mundane results produced by less prestigious scientists, unless they become critical as part of a larger research problem, are not likely to be replicated, and will consequently escape detection of error or fraud. Thus, the rank-and-file scientists, unlike other lower strata individuals, may never be exposed at all. It does not follow, however, that this will produce deviance among those individuals, as whether or not it does is an empirical question.

Consider sanctions next. The most important community sanction a scientist faces, outside of legal action if plagiarism is involved, is the loss of reputation and the inability to continue in research. The scientist with the most reputation to lose, therefore, if exposed, will receive the most severe sanction. And, in spite of informal discussions which assert that scientists are not really punished, there is considerable evidence to the contrary (for example, see Broad 1980c, 1980d, 1981a; Sun 1981; Wade 1981).

So in the social stratification of science, the probability of avoiding detection and the severity of being exposed disproportionately favor scientists in the lower strata. This is not equal justice, but it is a kind of justice not found in other institutions. And that fact, if publicized, should produce a greater sense of confidence in science by society in general for the opposite reasons why other groups and institutions may have suffered lowered prestige.

It should be emphasized that this discussion must necessarily look at the broad picture, even though many other dimensions are pertinent. First, the operation of science is quite different in the various disciplines and specialties. Several characteristics of the natural sciences, in contrast to the social sciences, produce a greater opportunity for deviant behavior to be detected and sanctioned: greater precision; more applicability; more researchers in a discipline and specialty; more competition for unique discoveries; less theoretical pluralism; less contamination from social change effects and observer influence; and more consensus on important questions. Because of these varying conditions across disciplines and specialties, gross generalizations about science must be subject to qualifications about applicability. These reservations should be kept in mind in further studies but should not inhibit current discussion.

In the current controversies, the problem of fraud requires several foci (see Broad 1981b): How much is there; is it increasing; does it have different origins under our changing social conditions? Most agree that deviant behavior is largely a product of social conditions, not individual pathologies. In the most comprehensive published review of the literature, Zuckerman (1977) did not see a really good fit between any general theoretical perspective in deviance and the specific case of deviant behavior in science. She suggested that the (then) current cases of deviant scientists did not seem to lack an advantaged position in the opportunity structure. She noted, however, that "This suggests that a marked emphasis on a collectively defined goal may be enough to 'attenuate conformity to the institutional norms' even among those who would seem to have privileged access to 'opportunity'" (Zuckerman 1977:128-131). While the notion of lacking the opportunity to obtain culturally desirable goals is fundamental to the anomie theory of deviance, it seems entirely reasonable that even if one is relatively high up in the opportunity structure there are still higher echelons to reach, and therefore it is possible that anomie theory, if focused on certain segments of the social structure, may indeed hold the key for understanding deviance in science.

SUMMARY AND CONCLUSIONS

The prevalence of contemporary deviant behavior in science is problematic, and to assume that it is increasing may not serve either science or the search for truth well. The critical issues, I believe, are: (1) how to deal with what deviance there is in science; and (2) how to manage the problem in such a way that the most negative consequences for the future of science in our society do not become a self-fulfilling prophecy. To resolve these issues, some institutional change is required.

First, greater attention in all disciplines should be focused on the general lack of replication and publication of such studies. This requires a change in attitudes about the important role replication can have. And the reward system, including the policies of publishers, must be altered to give some recognition for such work. The actual or perceived threat of having one's work replicated would increase the level of care and attention given to the research process and product. Although replication of significant discoveries is expected, the great majority of work will never be replicated given the current conditions in the financial support and reward systems.

Second, to maintain the integrity of science within its own community and in the society, the scientific community must not reduce, but rather increase its vigilance in identifying and reporting cases of deviant behavior. It is especially important to report the outcome of such self-policing activities so that everyone can know what sanctions were applied and how strictly they were enforced. While it is unfortunate and sad when any individual scientist has a career ruined, the Constitution does not guarantee anyone the right to be a scientist, only the right to due process. Universities sometimes function as social welfare agencies in personnel decisions, and it is difficult, both legally and emotionally, to cast out colleagues, especially from tenured positions. However, the rules are well known and the rights and responsibilities of faculty members are clear. (Besides capital punishment is not a proposed penalty, although self-execution is not unknown.) But some kind of institutional mechanism for investigating and publicizing the results--including deprivation such as revoking membership in professional societies--would seem appropriate. For as long as the community is able to remain self-policing, it must continue to do so with a vengeance since the alternatives are unacceptable.

In conclusion, scientists are human, so deviance cannot be eliminated. Occasional deviance and its exposure serve to remind us of this and to reaffirm collectively what the appropriate norms are. The objective, public, and swift handling of deviant cases is not likely to reduce society's trust in science. It is more likely that such practices will increase trust, and continue to permit the scientific community to handle its own problems--two fundamental requirements necessary for maintaining the level of autonomy desirable to enhance innovation in science.

ACKNOWLEDGMENTS

I acknowledge here the debt to scholars who, in various ways, have shaped my inquiries into deviance in science. Alphabetically, they are Bernard Barber, Joseph Ben-David, Diana Crane, Warren Hagstrom, Robert K. Merton, Derek J. de Solla Price, Norman Storer, and Harriet Zuckerman. None of them have read this paper, however. Additional thanks are due to the symposium presenters and participants for helping clarify some of my basic points.

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ANOMALY REVISITED: RULES AND ROLES IN RESEARCH

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This paper reviews, first, the varieties and sources of deviancy in scientific research. It examines, second, the researcher's work situation, regarded in terms of the status and role of the researcher, with special attention to the bearing of value conflicts on the commission of fraud. And finally, the present paper examines the consequences, both personal and institutional, of fraud in science; consideration is directed to the need for a better understanding of the human contexts of science as a social enterprise.

Recent Literature on "The House of Science"

The House of Science is the object of much and varied reportage these days, both within and outside of science. For many it is still, indeed, the "House of Intellect." But from another group, increasing in number, it is being reported as something different from our traditional picture of the House of Science (see Broad 1981b). They report a growing pattern of science research behaviors which never seem to have belonged to the Old House: cheating, data concoction, fact fudging, lying, fraud, among other malfeasances, which we are still hearing about (see Hunt 1981). One response from the scientific community has been, in the words of the highly regarded Philip Handler, that the situation is "grossly exaggerated" (see Broad 1981a); yet no one says the allegations are untrue, and no one has sought to make even some timid estimate of how much cheating in science research is really going on.[1] And what is even more significant: no one holds that the matter is unimportant. Indeed, so many different accounts are bringing to the surface so many variants of the pattern of deliberate, even devious, deception that a more general and meaningful term than cheating seems to be more and more appropriate. We are now being confronted with the high likelihood of _companies of deviancies_ in the House of Science!

Part of the difficulty which the scientific community has in coming to terms with the new situation lies in the fact that the reports of misrepresentation and distortions of data do not coincide at all with the traditional picture of the House of Science.[2] More so than in any other situation, these allegations concerning the field of science are regarded as keenly offensive. But another part of the difficulty seems to be that, though some of the fraud and fudging are rather fully reported, most of our information is still journalistic, which has its own problems in false reporting.[3] Deviance in science does, in fact, seem to be far from regarded as a major and enthusiastic area of important science research. So far we lack any systematic or empirical studies. Questions of prevalence, much less of incidence, remain, at best, therefore, guesses.

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[1] See, though, St. James-Robert 1976. While his survey of the readers of _New Scientist_ was, as St. James-Robert was the first to note, biased (i.e., non-random), his results suggest a potential prevalence of variegated fraud in the scientific community much higher than the more conservative estimate NIH director Donald Fredrickson gave Congress last year of a "dozen or so cases" known to NIH over the previous five years (see Broad 1981a:421). As Broad notes, some scientists and philosophers of science hold that fraud is ubiquitous and essential (Broad 1979, 1981b).

A quick review of the literature provides an ample range of reasons for the lack of enthusiasm for major research in this area.[4] The images of cheating in science present a clear and fairly persuasive range of evaluations:

1. the situation is a tempest in a teapot;
2. there are always rotten apples in any barrel;
3. the situation is like a drop of ink, diffusing through the system, so that the whole situation is deviant to some degree, or in some ways;
4. a lot of cheating, in any situation, must be seen as a simple and unusual matter, re: personality, the character of the situation, the research organization;
5. cheating is a function of certain phenomenal and epiphenomenal (that is, aspects internal and external to science) effects, all interrelated;
6. cheating is a function of a management pattern which has failed to be alert to, or has no basic estimate of, the cheating, or any managerial picture of sources, techniques, or controls over the routes and occurrences of data mismanagement; and
7. there are sufficient feed-back mechanisms in science which will, in due time, identify and correct the deliberate errors of deviant research. The science process is self-correctional (see Zuckerman 1977).

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[2] The traditional intellectual history of science as against the counter-traditional emphasis on social and economic aspects are both invoked in recent discussions of research fraud. Fraud in research has now become one of the puzzles in the view of science as intellectual puzzle-solving. On science and puzzle-solving, see Kuhn 1962.

[3] The subject of deviant research has attracted a strong journalistic reporting interest, but little if any scientific research interest. For an article dealing with journalism and the role of scientists, see Weinberg 1969.

[4] Titles which appear in the references indicate, far more than they demonstrate, reasons for the lack of enthusiasm. The usual theme, that science is self-correcting, is the chief rationale for the lack of great research alarm over evidences of deviant research.

It may be that all of these response evaluations are reliable to some degree. But research studies supporting or invalidating them do not exist, certainly not at a level sufficient for policy and management decisions. However, voices calling attention to the disturbing resemblance between these widely reported cases in science to those appearing in the literature on criminal justice are still rare (see Kilbourne, Kilbourne, & Higgins 1981). Yet even a quick perusal of E. H. Sutherland's classic study of white collar crime lines out a formidable pattern of similarities (see Douglas & Johnson 1977; Erman & Lundman 1978; Geis 1974; Johnson & Douglas 1978; Sutherland 1949).

At the same time, reports of offenses in science research continue to come along, such that anger replaces hand-wringing lamentations. The pharisaical piety that "we are not like other men" is now less persuasive than the unhappy confession that "we are indeed now like all other men." Moreover, the trend in the literature has taken a turn: what were once "problems" of science research are now being described in terms of a "problematic" of science research.[5] The difference is dramatic and different. In the earlier reports, we were reading about lamentable, regrettable, even tragic situations of human failure, perhaps, it was thought, less common to science than other institutional fields. However, the newer reports describe a range of failure that suggests that there is now a problem in science research *sui generis*, a failure that questions the credibility of science itself. The cases are defining, or at least pointing to, some new dimensions in contemporary science research. This new orientation is important because it suggests that the cases of research deviancy are structurally conduced; that for this reason the deviancy in research is subject to remediation; and what is even more important is that the whole deviance scene can tell us some valuable things about contemporary science which we really need to know and understand.

Part of the interest in deviancy in research is suggested by the suspicion of something which has gone on for a long time, but which did not really have much impact, if any, on the historical development of science (see Broad 1981b). That is, deviancy in science research is not new in the history of science (see, for example, Bateson 1923, 1929; Broad, 1980a, 1980b, 1980c, 1981d, 1982; Carter 1979; Culliton 1974a, 1974b; Dorfman 1978; Gillie 1979; Goldschmidt 1949; Hunt 1981; Noble 1926; Przibram 1923, 1927; Rimland & Munsinger 1977; Shapley 1972; Straus 1954; Wade 1973, 1977b, 1981a; Westfall 1973; Zirkle 1954). Awareness of its existence has long been privately shared before it became publicly displayed; a private story before it became a public scandal. What is really new now, perhaps, is the public exposé of cases and types of research deviancy in place of privately circulated droll accounts of long forgotten and forgiven misdeeds.[6] What is also really new is that a public forum has emerged; a forum consisting of Congressional hearings, committee investigations, and those journalistic reports which have subsequently enlarged the range of activities, the cast of characters, and the intended audience. Thus, Charles Babbage's typology of fraud (Babbage 1830) named only a handful of deviancies: (1) *forging* (fabrication of data); (2) *trimming* (manipulation and cutting bits off); and (3) *cooking* (choosing data that fits the hypothesis, discarding data that does not fit). Later accounts have developed their own air of familiarity:[7]

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[5] The distinction here is not pedantic. It describes a shift in realization: that problems which are being described themselves constitute, by reason of their controversial nature, a major theoretical and methodological issue.

1) *refusal or inability* to replicate original studies;[8]
2) *concoction*--creation of fake data;
3) *cover-up*--administrative dawdling, engaging in denial or ambiguities, avoiding and prolonging investigation (see Broad 1982);

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[6] This public awareness is a function, in many ways, of the rise and development of scientific journalism, as well as the new themes developing in both the history and sociology of science. Broad notes, on this point, that the National Association of Science Writers had 15 reporters when it was founded in 1934, and had over 1000 members by 1981 (Broad 1981b:139). Another indication of increased public awareness is the rise in the number of political action committees (PACs). Between 1974 and 1980, the number of PACs went from 608 to 2551, or a percent change of 319.6% (see Bergmann 1982).

[7] Reading the accounts in recent years, one is impressed by an almost predictable pattern or format of events, complaints, defenses, evasions, avoidance.

[8] As Broad points out, however, there is a new situation in science where replication, while theoretically possible, is economically infeasible (Broad 1981b:140). One attempt at a cure for the type of fraud that occurred in the Straus affair at Boston, he notes, was the establishment of random audits. Related to this is the deviance that occurs in the drug industry. With a great deal at stake, errors in research testing tend to get covered up (see, for example, Smith 1977a). Here we see a structural conduciveness to deviancy with an emerging pattern: errors in the research process, coupled with high stakes and the need to integrate research with other productive aspects of organization (part of the industrialization of research, whether it occurs in industry or elsewhere), result in deviant acts to cover them up. In industry there is, of course, high investment, long term planning, beating the competition, patents, etc. (see Galbraith 1967). In a non-industrial setting, the stakes are just as high as science has emerged from a vocational to a career pattern. Reputation, success, money, prestige--the scientist can lose it all, especially the young rising star, with an error. The career becomes, in a manner, a product. Its steps are anticipated, planned for, and integrated with the other aspects of institutionalized science. This was a pattern present, for example, in the John Long case at Mass. General (see Wade 1981a) and the Summerlin case at Sloan-Kettering (see Culliton 1974a, 1974b).

4) *plagiarism*--via peer review system and other devious but known devices (see Broad 1980b, 1980c, 1980d, 1980f);
5) *punitive administrative acts* which range from mild reprimands, demotions or transfers, to dismissal;
6) *collective mea culpas* by appropriate administrative officers which claim ultimate responsibility for a situation which will never happen again;
7) *continuation of grants* to agencies, institutes, and other collective devices of research with a record of such offenses, as well as to individuals, and with little or no structural organization to prevent or detect or even deal with deviancies (see Broad 1981c).

In brief, a mixed lot of unorthodox research activities has been going on in the stately old House of Science.

Fortunately, some of the response in the scientific community has moved from a recognition of the seriousness of fraudulent research in the name of science to a recognition that an understanding of the individual malefactor of faked reality must be linked with the recognition that there are structural factors which participate in these dramas of deception, factors which indeed encourage--perhaps the better word is conduce--these instances of misrepresentation of reality.[9] Table 1 relates areas and problems of concern with scientific deviance to both personal and institutional sources (that is, phenomenal and internal factors) of science, on the one hand, and contextual sources (that is, epiphenomenal and external factors) of science, on the other hand.[10] The following list of sources of deviancy brings together interpretations and explanations which appear in an extensive literature cited in the references at the close of this paper.

Table 1. Sources of Deviancy in Science Research

Phenomenal/Internal	Epiphenomenal/External
I. Institutional	
Advocacy of findings;[11]	The pattern of institutional

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[9] This statement seeks to emphasize the shift in historical and social research to continuities and total context in depiction of a set of events.

[10] The term epiphenomenal is used in the philosophical sense of *contextual*, not in the sense of trivial or mere appearance.

[11] On this point, see Polanyi 1962, Chap. 6:132-202. See especially, subsection 2, 'Scientific Value,' pp. 134-142; subsection 3, 'Heuristic Passion,' pp. 142-145; and subsection 5, 'Scientific Controversy,' pp. 150-160. Polanyi notes that a scientist does not just set forth the fruits of his labor. He sells it, advoates it. His advocacy is part of its reception and evaluation. He is not neutrally involved, and he has something to lose and gain--prestige if it is accepted and loss of prestige if it is not. Advocacy, of course, has

Table 1. (Continued)

Phenomenal/Internal	Epiphenomenal/External
Inadequate accountability system; Inadequate replication system, especially connected with large-scale research which cannot be replicated due to costs; Inadequate standards on data retention; Exponential leads and lags in many aspects of science; The professionalization of science: from science as vocation to science as career; Declining role of mentor in the education of novitiate scientists; Publish or perish (or publish and perish) -- the problem of paper inflation, data fragmentation, vitae inflation, etc. (Broad 1981e; Reif 1961); Perpetuation of the "star system" in contexts that are increasingly inappropriate; Concentrations and/or monopolies in education and research: inadequate mechanisms to insure "meritocracy" in science (Caplow & McGee 1958; Cole & Cole 1972; Crane 1965; Kash, White, Reuss, & Leo 1972; Merton 1968a; Wade 1978); Increasing bureaucratization of scientific research and proliferation of rules; Hypertrophy of prestige of research and hypotrophy of teaching prestige.	recognition and rewards; The institutional scramble for scarce research funds; Increased articulation with other major institutions -- political, economic, military, etc. (see Kidd 1959; Melman 1970; Mills 1956; Nelkin 1972; Nieberg 1970); Increased infusion of conflicting values into research: proprietary interest, vested interest, patents (Wade 1976, 1981b); Inadequate legal protection for the "whistle-blower;" Irrationalities in the funding process; Increased emphasis upon secrecy -- national security, proprietary interest, fear of being scooped, etc.; Over-emphasis or even mandate to produce results: e.g., cancer research; Politicization and even polarization of scientific personnel (Boffey 1976, 1982; Broad 1980e; Dupre & Lakoff 1962; Rossi 1964; Snow 1961, 1963; Wade 1977a); Budgetary cuts and inflation; Mixing of funding sources, with potential conflict of interest (Carter 1975; Shapley 1973; Wade 1972a, 1972b, 1972c); The industrialization of research (Meadows 1950, chap. 2, 1951, 1965, chap. 4)

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different degrees, from British reserve to Nobel fever, and has many, often complex motivations--advancing science; advancing truth; money; prestige; getting a Nobel, a teaching position, a place in the NAS; beating a rival; and many other motivations (see, for some examples, Merton 1969; Wade 1981c; Watson 1968).

Table 1. (Continued)

Phenomenal/Internal	Epiphenomenal/External
II. Personal	
Increased role complexity and over-commitment; Inadequate recruitment and selection; Inadequate, incomplete, or inappropriate socialization for scientific role; Increased pace of scientific life -- physical and emotional exhaustion, breakdown, etc.; Increased technical and methodological sophistication coupled with decreased logical sophistication (Ziman 1968); Inadequate public visibility of research results; Increased exposure to situations where a value choice has to be made: i.e., increase in role conflict; Increased cost of failure as science becomes a career: the stakes are higher.	Old-boy network; Peer review system (Crane 1967; Gustafson 1975; Hunt 1981; Zuckerman & Merton 1971); Inadequate supervision; Emergence of large and varied client system.

Extended Research into Scientific Deviance

The preceding discussion was intended to establish--if that is the correct word for it--the need for a rationale for serious research, carefully thought through and gallantly supported, into the scope and nature of cheating in science research.[12] Deviants we seem to have and, perhaps, also some innocent victims. Unfortunately, research commensurate with the dimensions and significance to science research of these deviancies, oriented to the questions which such deviancies arouse, and, above all, framed to assess the needs and possibilities of responsible reaction on the part of the scientific community, we do not have. What we should like to do, at this point, is to write a scenario, not only for the rationale for such research, but also for the assessment of findings.

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[12] There is, as indicated, little empirical data on scientific fraud. Like white collar crime in general, it is a difficult area for empirical investigation. There are, however, some preliminary steps that could be taken. The use of surveys might be one, random or otherwise. Note has been made of St. James-Robert's survey in New Scientist (1976). Although this type of survey is not random, it is, as an exploratory instrument, informative. Random surveys, stratified by structural and other variables, could also be used. Some data already does exist (e.g., FDA, NIH, Congress) and could be systematically explored. A public forum to discuss the problem and issues would be useful as well as lending legitimacy to illegitimacy studies. Citation analysis might be used to follow the diffusion of known fraudulent results. Known offenders might also be interviewed. There are, of course, many other possibilities. For some previous studies on cheating, see: Bonjean & McGee 1965; Bowers 1964; Harp & Taietz 1966;

The preceding table on sources of deviancy makes it clear that any research into deviancy will wind up considerably shortchanged if it concentrates exclusively on the individual research worker. Deviancy here, as elsewhere, is both phenomenal and epiphenomenal.[13] That is, as indicated in Table 1, deviancy must be regarded in terms of the internal and the external aspects of science. Such an orientation suggests that deviancy, by type and frequency as well as process, will vary, like any behavior, with the context. Discovery of such variations would be of great concern. The factors bearing on such research are suggested in the following table, which brings them together in a functional way in terms of social context and research role. It should be noted here that research is defined in terms of both role structure and organization. The scientist may work solo or as a member of a team. In terms of his organizational context, the research situation varies with the degree of organizational complexity, identified in Table 2 simply as high and low. Whether actual incidence or prevalence of deviancy in science varies with each of these dimensions must yet be determined by empirical research.

One implication of Table 2 is that research conducted along these lines would be, in the history of science at least, a bit extraordinary, for it would be in fact a collective self-analysis. Beyond that, it would move the focus of investigative interest from purely psychological and personalistic factors (as well as the technical and methodological) to the social and cultural factors which have come to be seen in recent years as a major aspect of modern science (see Barber & Hirsch 1962; Barnes & Dolby 1970; Ben-David 1976; Bernal 1946; _Daedalus_ 1963; de Gré 1955; De Solla Price 1963; Elias 1971a, 1971b; Fleck 1979; Goldsmith & Mackey 1964; Greenberg 1967; Haberer 1972; Holton 1960; Kaplan 1962, 1964; Kuhn 1968; MacIver 1961; MacLeod 1977; Merton 1941, 1970; Perry 1966; Richter 1980; Rothman 1972; Ziman 1968; Znaniecki 1940, 1955). Much of the reason for the shift lies, as we shall argue in the final section of this paper, in what may be summarized as _the industrialization of scientific research_ (Meadows 1951, 1965). Such a transforming orientation obviously links problems and limitations of scientific research with the organizational contexts of

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Hartshorn & May 1928; Stouffer 1949. On the use of the computer and cheating, see Clines 1982.

[13] Both the deviance and white collar crime literature distinguish between individual-personal factors and contextual factors, between events and processes. The distinction also separates out moral evaluation and guilt assignment (cheating in science as a problem) and contextual-structural analysis (cheating in science as a problematic).

Table 2. Organizational Context

Research Role	Organizational Complexity	
	High	Low
Solo	research specialization; extensive individual task assignment; multiple levels of administration.	research singular, independent; frequently low funded; informal management; flexibility in scheduling, deadlining, reporting.
Multiple	research multiple and specialized; high supervision level; diversification of research projects & tasks; varying patterns of group integration.	premium on individual creativity and spontaneity; functional coordination of work; administrative personnel informal, nondirective, rewarding.

industrial production, contexts which have been examined extensively by the social sciences in the last few decades.

This structural overlay of scientific research with industrial organization for production in general suggests a number of salient leads for an extended research into deviancy in science research. Three such leads will be described very briefly. First, cheating, like a number of abnormal or normal events in any organization of production, cannot be seen merely as a single, isolated event, occurring independently or haphazardly, but as part of a larger process of action within an organizational framework of action.[14] Second, a distinction becomes essential between deviancy as a social action and the individual deviant involved in--perhaps caught in?--deviancy. Third, in the work of Robert Merton, among others, it has been suggested that any social situation of action is characterized by a mixed formation of differentiated and usually conflicting norms of behavior, held in an uneasy balance which can be tipped by a variety of factors (see Merton 1968b). This sociological ambivalence can be reinforced or diminished by structural decisions and changes which, unfortunately, may or may not have a direct bearing on the pressure toward a fraud (on ambivalence, see Merton & Barber 1963). For we are dealing in research deviancy with a continuum of possibilities, not with an inherent polarization or clear-cut dichotomies. Day by day demands in research work easily reflect, but without necessarily displaying with acute consciousness, the intense and simultaneous pressures of norms, ab-norms, and counter-norms of the work place. Indeed, as social science studies have increasingly shown, such a possibility is itself frequently a function of the lack of any real or significant integration of values (see Allport, Vernon, & Lindsey 1960; Barrett 1961; Edel 1965; Kluckhohn & Strodbeck 1961; Fletcher 1966; Rescher 1966, 1969; Williams 1970). Thus, a decision to engage in a violation of an ethical norm may easily occur in a hierarchy of values in which technical norms dictate the priorities and directions of decision. In such a situation, to lie or cheat may appear to be a matter of no moment. On the whole, despite the extensive literature on values and norms, there remains a baffling dearth of studies of conflicts in norm-oriented decision.[15] The literature on bio-medical ethics is an outstanding exception (see Reich 1978).

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[14] For elaboration of this theme see literature summarized by Farrell & Swigert 1978; Liska 1981; McCaghy 1976; Rubington & Weinberg 1968; Taylor, Walton, & Young 1973; Wilensky 1967.

It is not easy to make a straightforward identification of fraud in research with types of norms because situational factors create great overlap, and the norms themselves are not without ambiguity. However, since value-orientation is at the heart of the issue, some attention should be paid to typological as well as situational aspects of research fraud. Table 3 suggests some of the possibilities for research fraud.

Table 3. Research Fraud

Type	Instances
I. Ethical	False credentials; Plagiarism; Falsified grant proposals; Lack of verifiable citation; Misuse of peer review system; Maltreatment of research subjects (see Smith 1977b, 1977c).
II. Technical	Data fudging; Data concoction; Improper research protocols (see Holden 1979; Smith 1977a); Improper or misleading use of statistical tests and procedures.
III. Economic	Pooling of funds (see Shapley 1977); Falsified accounts; Stealing equipment/money.

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[15] There is a large literature on role and role conflict. Three papers that serve as a good introduction are: Getzels & Guba 1954, 1956; Goode 1960b. For a good collection of papers on role theory, see Biddle & Thomas 1966. For further reference, see Goode 1960a; Gross, Mason, & McKachern 1958; Stouffer 1949; Toby 1952.

Moreover, a crucial aspect of research into research deviancy concerns the matter of intent, a primary problem in any instance of alleged fraud. Motivation here is itself an interesting panorama of familiar possibilities: fraud (1) in order to gain something; (2) in order not to lose something; (3) as a prank or game or both; (4) in order to relieve boredom, as in hired hand research (Roth 1966); (5) as a labor-saving device (i.e., cutting corners, a few fudged numbers here and there will be averaged out); (6) as functional and dysfunctional; (7) as trivial or neutral; (8) as a rear-guard action; and (9) in order to cover up sloppy or contaminated techniques.

Assessment of Consequences

One of the things which students of John Dewey learned about human action was the way in which consequences, intended or unintended, enter into the adoption of ends and means (Dewey 1931; Edel 1961; von Wright 1963; Weber 1949). To focus on ends and means without finding their linkages, before and after, with consequences severely truncates the research and leaves the problem situation in a condition which can only be depicted as status quo ante.

Assessment centers not only on the "human meaning" of research deviancy, but also the "human meaning" of research deviancy for generations of workers to come. It further extends to the House of Science itself. The key question becomes: is our "taken-for-granted" knowledge of science adequate to, or even relevant for, the implications of research deviancy for science itself?

In this concluding part of our paper we shall, once again, raise only some salient situations of concern. Assessment is a research problem in itself. It is no mere matter of pulling out some ethically oriented threads from the cloth of factual observation. What follows falls within a research pattern of a familiar functional form: if these X_n, then these Y_n. Moreover, the concerns underlined here are based on the assumption that the fraudulent management of research processes and products is both a moral and, in some situations, a legal matter, with consequences for the public welfare of science as well as that of the general public. It represents a situation which, by any criterion, mandates assessment, and one which surely goes beyond knuckle-rapping and verbal remonstrations.

First, correctional actions, if any, should be addressed equally to the phenomenal and the epiphenomenal--to the internal and external--aspects or sources of deviancy in science research. Evenhandedness is as imperative here as in any situation of joint responsibility for results. Of course, this requirement may create a role strain, for example, in management and administrative quarters, where the prevailing climate of moral opinion often is a petite Eichmanism (i.e., "we only work here"). Responsibility here includes concern not only for the efficiency of deviancy detection but also for the safety and security of the "whistle-blowers," who are often the innocent victims of somebody else's fraud.[16] To put the matter in the large pattern of meaning it deserves, we can foresee that we may come, in the not too distant future, to the perception of the need for a moral casuistry for the guidance and education of research administrative and operative personnel as important as the customary agenda for technical education.[17]

Reference to a moral casuistry for science, already the focus of the rapidly growing field of bio-medical ethics, underscores another area of need for assessment research, one which would take another look at the famed Kuhnian paradigm of normal science (Kuhn 1962). Kuhn's paradigm in no way suggests that fudging, trimming and lying are elements of the operations of normal science. The epistemic-methodological concerns of his volume center on anomaly and on the tensions developing from the anomalous circumstances that some facts do not fit the ascendant pattern of explanation or style of interpretation. Interestingly enough, absence of concern with fraudulent research in normal science in Kuhn's study is itself an anomaly. Exposés of deviance in research indicate that there is here a valuational anomaly, fully as important as Kuhn's theoretical and methodological anomaly. Put pointedly as possible, there is in normal science a research situation in which judgments on action appeal to a set of values which normal science implicitly prohibits and outlaws, but also implicitly permits, even encourages or conduces, and appears to be unprepared to do anything about.[18] Such a situation, of course, is not a revolution in science in the making, which is the point of Kuhn's valuable analysis. On the contrary, it seems much more like business as usual.

The reference to Kuhn's paradigm, moreover, raises the very appropriate question of whether the formulation of a statement of universals about science, such as that by Robert Merton, does not, in fact, maintain a comfortable distance from the annoying counter-indicators of everyday science (see Greenberg 1966; Merton 1968b, chap. 18, 1975). So comfortable, in fact, is its distance from the daily particularities of science research that the Mertonian list of universals represents a litany of pieties to be saluted, as in the grand public ceremonies, with great gratification, before the real game gets underway. As a minimal statement, we might agree that the Mertonian statement of science universals (including its emendations and additions) is incomplete. Yet at the same time, the problem-situation which prompted his formulation of the statement of universals is surely as urgent, if not more critical now than ever.[19]

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[16] On the question of the whistle-blower and some of the problems he faces, see Broad 1978; Holden 1980; Wade 1972a, 1972b, 1972c.

[17] Indeed, one of the problems is that no one knows what to do with the scientific cheater. There is no real role structure for either the cheater, his detector(s), or his judges. Hunt, for example, in discussing the affair at Yale involving V. R. Soman and P. Felig (an affair that included misuse of the peer review system, plagiarism, and faking of data), says of the subsequent "scientific audit" done by Dr. Flier: "there are no rules by which to conduct them..." (Hunt 1981:42).

[18] Ironically, the highly touted "value-neutrality" of science finds itself in great trouble in the moral issues arising from research deviancy. "The stone that was rejected by the builders has become the cornerstone."

Social scientists, including Merton himself, have been long aware of the complicated relationships which exist between legitimate and illegitimate ends and means in science research (see Cloward 1959; Cloward & Ohlin 1960; Merton 1959, 1968b). There is indeed a considerable variety of ways in which they can be combined and subjected to permutations and combinations, resulting in severe confusion for the decision-maker. Little attention is paid, in the socialization of recruits to science or participants in science organizations, to this situation of large-scale ambiguity in social actions of all sorts, and one which is so generative of the very deviancy on which our worries about science focus.[20] Clarification of these ambiguities is a research problem requiring an immense imagination, one sufficient to match the immense complexity of this interface between science creed and science custom, between the value-neutrality of scientific methodology and the value-freedom of research deviancy.

Even though there are undoubtedly elements of ambiguity in the investigation of yet another set of consequences of research deviancy, the issue of costs (or perhaps risks) arising from such deviancy is clearly a consequence which needs some attempt at assessment. Some kinds of risks are suggested in Table 4.

Finally, the assessment of consequences, as part of the extended research which our general problem invites if not mandates, requires that we pay attention to some implications which go beyond the flagrant act of research fraud itself. Historians and sociologists of science have made it unmistakably clear that the House of Science is not merely a House of Intellectuals but of real human beings. Science is, beyond the mere logical and methodological, beyond the epistemological and technical nucleus of scientific tradition and research, also a social enterprise, one which should be examined and investigated as a social enterprise, and one which on occasion is subject to revision and reform like all other social enterprises (see Meadows 1949).

However, such review and revision always seems to raise the question of whether the job should be done by insiders or outsiders (see Becker 1963; Merton 1972). The answering premise usually favors the

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[19] For an attempt to extend these norms, see Mitroff 1972, 1974a, 1974b, 1976.

[20] For an interesting example of combining intent with outcomes (behaviors, actions), see Blake & Davis 1964:468 and Table 1.

Table 4. Risks of Research Deviancy

Phenomenal	Epiphenomenal
Exit of talented persons from research; Loss of funding for important research programs; Loss of personal autonomy of research scientists; Decrease in trust and increase in suspicion among research scientists; Loss of talented persons from research (the deviant is no longer productive); Wasted time, money, and effort; Damage to reputation of innocent persons; Loss of institutional prestige; Loss of potential talent which chooses a career other than science.	Loss of prestige of science and scientists; Use of science as a scapegoat; Waste of valuable and scarce resources that could have been spent elsewhere; Loss of lives; Wrecked lives (e.g., unethical medical experiments); Potential criminalization of some aspects of science; Environmental damages.

insiders, though why it could not just as simply include knowledgeable outsiders is as mysterious as much of what goes on in deviancy itself. In any case, it is futile to quarrel with these insistences. What is so provoking, however, is that so frequently in the history of professional organizational self-inventories and assessments something significant, emphatic, and lasting seems seldom ever really to happen. Perhaps here, in the matter of research deviancy in science, as in other social enterprises, time is the answer: that in time self-correction will be developed and in time will be set in place. We are fully empathic with this collective hope, but it is an empathy without full persuasion.

SUMMARY AND CONCLUSIONS

In summary, then, we can make the following observations concerning science and deviance. First, the study of deviance in science has barely and recently begun to move beyond a journalistic and reportorial stage. The study of deviance in scientific research has yet to become legitimated. Second, it is suggested that research into deviancy in science will fail to move beyond such a fragmentary or unsystematic stage if structural or contextual factors are ignored. A dominant view has been to regard deviance in science as an individual phenomenon. The danger for science in such a conception is twofold. An individualistic approach allows scientists to ignore the question of deviance in the scientific community. If it is seen as an infrequent problem, and as one involving only occasional individual failure at that, it hardly requires extensive research. Random events are simply lived with, not explained, coped with, not changed. It also replaces understanding in science with apologetics or polemics, and allows outsiders who are hostile to science to utilize science as a scapegoat. A structural or contextual approach, however, has the clear value, as studies of deviancy in other areas have demonstrated, of broadening and enriching the view that deviancy is not an isolated event but part of a pattern.

Third, it has been suggested that within a structural or contextual framework it would be useful to approach deviancy in science in terms of the structural overlay of scientific research and the industrial organization of production (i.e., the industrialization of scientific research). This orientation places scientific deviance within an organizational framework of action. It also makes the question of ends and means, of norms and values, very salient. In particular, the question of competing and conflicting norms and values in the "doing" of science becomes central. It has been suggested in this presentation that the lack of any significant integration of values may allow technical norms to obtain priority over ethical norms. The ambivalence, the abstractness, and the ambiguity of the customarily cited norms of science, noted in this discussion, have an important bearing on both the incidence and prevalence of deviancy in scientific research. It is an unpleasant irony that values officially, ruled out of the House of Science, have quietly re-entered, even though through the back door.

Fourth, it was suggested that deviancy in science research represents a situation that mandates assessment. As a research problem, assessment was noted to involve the following with regard to deviance in science: (1) The fraudulent management of research processes and products is both a moral and, in some cases, a legal problem. (2) Correctional actions should be evenhanded. There is a responsibility entailed in the detection of research fraud. Detection has a dual aspect: (a) efficiency in detection and (b) protection of the innocent victims of someone else's fraud. Note was made, for example, of the need for the protection of the "whistle-blower" who, at the present time, may risk a great deal (see, for example, Broad 1978; Holden 1980). (3) The development of a moral casuistry for science. This, too, has a dual aspect. Such a development would serve at least two purposes: (a) the education and guidance of administrative and professional personnel in how to detect and deal with science deviancy, and (b) the more complete socialization of scientists and novitiate scientists. It might also serve the additional purpose of creating greater public confidence in science. (4) A re-assessment of Kuhn's paradigm of normal science. It was noted that the absence of concern with fraudulent research in normal science in Kuhn's study itself constitutes an anomaly. Concerned as it is with the cognitive structure and history of science, it ignores the valuational aspects--in this case, the presence of a set of values in science which both outlaws and permits deviance. Such a re-assessment would involve, at the least, a re-interpretation and re-statement of Merton's analysis of science universals (Merton 1968b, chap. 18). (5) Finally, the assessment of deviance in science raises the question of who shall do the job. Should it be done by insiders or outsiders? The answer to this question is not fully within the hands of the scientific community. As indicated earlier, there has been an increased public awareness and interest in deviance in science. Some of the reasons for this are inflation, budget-cuts, the rise and increase of scientific journalism, the increase of political action committees (see footnote 6), and so forth. But whatever the reasons, the question of insiders and outsiders is salient. How the scientific community responds, or fails to respond, to the question of who shall do the assessing will have important consequences for science and science research.

In conclusion, then, it is useful to recall an observation by Suzanne Langer that the recognition of error is the beginning of intelligence. One is tempted to add that the reduction or prevention or, upon some deserved occasions, punishment of error, of error by design of course, is the beginning of wisdom. If error is the mortal enemy of the progress of science, deliberate error is the destructive agent of its integrity--and its credibility. It is the course of wisdom, surely, to *know* and to *do*: to know the incidence, the prevalence, and the processes of deliberate deception in scientific research, and to take those actions that would reduce, if not prevent it. Action is the whole point of the knowledge; otherwise, the House of Science adds yet another piety. If replication cannot root out deception, if research audits cannot either, if detection systems cannot work, and if the management of the structural factors which are conducive of fraud fails, at least it can be said that scientists have not acquiesced in their own degradation.

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PART THREE

The Administrative Response to Deviance in Science

Part Three focuses primarily on what can be done about scientific misrepresentation and misconduct in the university setting and in relation to the continued Federal support and oversight of publicly funded university research. The detection, investigation, and prevention of scientific fraud by universities themselves is the specific concern of Barber in Chapter 6. In Chapter 7, McCarthy discusses the Federal response (NIH) to scientific misconduct and emphasizes the past cooperative relationship between universities and the Federal government. Dong takes an oppositional stance in Chapter 8 to the Federal government's "laissez-faire" attitude toward university self-regulation of research. He argues that past NIH policy has failed to protect the public interest, and he recommends specific action be taken in the future concerning both the investigation and prosecution of scientific misrepresentation.

87

FRAUD IN SCIENCE: WHO PATROLS AND WHO CONTROLS?

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My interest in science in general and science fraud in particular comes from twenty years of laboratory experience as a cell biologist and a decade of experience as a university administrator. As both a scientist and an administrator, I find that one of the publications that I read routinely is the AAAS journal, Science. The major difference in the two roles, however, is that as a practicing cell biologist I turn first to the Reports section of Science to see what new research is being reported, whereas as an administrator, I turn to the News and Comments section first to see what new fraudulent activity is being reported.

The subject of fraud in science has attracted the attention of many people both inside and outside science, including members of the Congress.

On March 31 and April 1, 1981, the Gore Subcommittee on Investigations and Oversight of the House Science and Technology Committee held hearings on the subject of falsification of research data. The Committee took testimony from many people including Dr. Donald Fredrickson, the then NIH Director, and Dr. Philip Handler, the then President of the National Academy of Sciences. They also heard from several individuals associated with reported cases of fraud. Among the many questions asked at those hearings were the following:

> Are leading scientists who run large laboratories paying enough attention to the work actually being carried out?
>
> Is the peer review process working adequately?
>
> Is science really self-correcting?
>
> Is the current medical education process creating an unhealthy and inappropriate sense of competition?

These are important questions and are typical of those being asked by concerned scientists, the media and the general public. There are no easy answers to these questions even though many people have strong opinions about them. The reactions to fraud that I hear from both my scientific and administrative colleagues indicate that science remains healthy, and that fraudulent behavior is as unacceptable today as it has always been. On the personal side, I must say that I find fraudulent activity revolting and, when discovered, it should be investigated expeditiously and the perpetrators must be dealt with forcefully.

A review of the recently reported cases of fraud in science chronicles examples of data fabrication and plagiarism (Broad 1981). Although acts of fraud appear to be relatively rare, their very existence erodes the public trust of science and scientists and serve to weaken the very principles on which science depends. I leave to others the debate on whether acts of fraud are increasing and whether its causes are related to the basic structure of science or the present environmental conditions in which scientific activity is supported and carried out. I limit this article to the pragmatic aspects of the problem, and offer my views on four questions:

1. What are the areas of potential fraud?
2. How can fraud be detected?
3. How do, or should, academic institutions inquire into allegations of fraud?
4. What can we do to prevent fraud?

These questions need to be addressed by scientists and administrators if we are to protect the environment required for performing science while preventing further erosion of the trust that society has given us.

What are the Areas of Potential Fraud?

Fraudulent activities tend to fall into a small number of relatively distinct categories including falsification of data, plagiarism, abuse of confidentiality, and deliberate violations of regulations (Broad 1981; Wade 1981a, 1981b). Examples in each category can be found in recent reports on this subject.

Of all the activities associated with cases of alleged fraud in science those related to the collection and reporting of data appear to be most prevalent. Falsification of data is of major concern because such activity is incompatible with the basic processes on which science depends. It also weakens the principles on which the self-regulation of science by scientists depends. Falsification of data takes many forms and includes a spectrum of unacceptable activities ranging from sheer fabrication (i.e., not doing the experiments being reported), through selective reporting (i.e., not reporting conflicting data), to data manipulation (i.e., fudging). Scientific progress depends on the accurate collection, analysis and reporting of data. Falsification misleads others and results in the waste of resources, both human and monetary. If practiced in areas such as clinical research it could also be dangerous to humans.

The rewards of science are associated with the recognition received from unique contributions made by the practitioners of science. Plagiarism is therefore especially fraudulent since it is an attempt by one individual to receive credit for the work of someone else. Outright plagiarism in areas of research that are actively pursued is generally detected rather quickly and appears to be relatively rare. In addition, punishment for its perpetrators has always been swift and harsh. Inadequate citation and parsimony in referencing may be tolerated, however, more than they should be. However, such inadequacies are likely to be perceived as moral questions rather than fraudulent acts (Hillman 1981).

Abuse of confidentiality is a significant act of fraud in an environment that depends on extensive peer review and open communication of research results. Although somewhat analogous to plagiarism, it is quite distinct. It is more difficult to detect abuses of confidentiality in most instances since it does not generally involve verbatim duplication of another's work. It deals more with the expropriation of another's intellectual property. In the present environment of federal support of research, ideas are openly discussed in research proposals submitted to federal agencies and these ideas are critically evaluated by the peers serving on review panels. Extensive data are usually included to support the ideas contained in these proposals. The ideas and preliminary data contained in research proposals are usually reviewed by departmental colleagues, university committees and administrators, as well as by the professional peers who serve on the agency review panels. In addition, completed research studies are normally discussed extensively with colleagues before they are submitted for publication in professional journals, where they are subject to further review by professional colleagues long in advance of eventual publication. Research results are also often presented at seminars and conferences prior to publication. The opportunities to abuse confidentiality and/or expropriate data arise at many points during these processes. Moreover, abuse of confidentiality can occur not only by the action of the primary reviewers but also by the action of those with whom these reviewers have shared the privileged information.

In many ways confidentiality is the easiest to abuse and is the most difficult to detect. If abused extensively, such behavior could result in a reduction of the openness with which scientists have traditionally shared their ideas and results. The ethical considerations that follow from such changes in the openness of science, for whatever reasons, have been discussed by Bok (1982). The increase in the proprietary interest of data in financially lucrative research areas, and in the government's interest in restricting free exchange of ideas emanating from certain research areas that are considered to be important for defense purposes, has also focused attention on the moral questions related to the way scientific information is disseminated or protected (Nelkin 1982). It is therefore not unexpected that abuses of confidentiality have been considered fraudulent scientific behavior.

A seemingly deliberate violation of regulations applicable to research was among the first cases of an alleged fraudulent act recently reported (Wade 1981a). There are many federal, state and institutional regulations applicable to research and many are changed at relatively frequent intervals. Examples of such regulations that have become codified in Federal Regulations include those applicable to the protection of human subjects, the use of recombinant DNA molecules and the use of animals. In spite of the responsibilities on the investigator and the institution for complying to an increasing number of such regulations, compliance appears to have been excellent since only a small number of cases of non-compliance have been reported by those at the National Institutes of Health responsible for monitoring such compliance.

In addition to fraudulent activity, there are several categories of activity that have been identified as areas that give rise to moral questions related to the behavior of scientists. Three categories are the inappropriate use of multiple authorship, the premature reporting of data and the delayed reporting of data.

Multiple authorship is of concern because it provides the opportunity for individuals to receive inappropriate credit for research work that they may not have carried out nor properly understood. Consequently, it is important that each author listed on a publication assume full responsibility for all material included in that publication. This issue assumes great significance because in the evaluation process used by our institutions for the purposes of advancement on the basis of merit, research contributions are important criteria. One might view abuses in this category as cases of the "falsification of bibliographies" instead of the "falsification of data."

Premature and delayed reporting of data are also viewed as potentially deviant in certain circumstances since the progress of science depends on open and prompt communication of reliable results. Although there are legitimate reasons for early and delayed reporting, there is concern that the potential for a conflict of interest in commercially important research can become the major reason for delayed reporting. Such a reason is obviously unacceptable when considered from the point of view of the structure of science.

How can Fraud be Detected?

The potential for detecting fraud exists at several levels. The following are several of the more obvious ones in the present research environment.

> Failure to repeat experimental data. In research areas of intense competition, this method of detection appears to be most likely. It occurs most commonly when workers in the same or other laboratories are unable to confirm research results reported by others (Broad 1981). One of the benefits of the openness of the research process is that it provides a mechanism for the verification of data. The opportunity to confirm results must be encouraged, and all results should be freely challenged both from inside and outside the laboratory. Secrecy, therefore, becomes the antithesis of the scientific process.
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> Co-workers in the same laboratory are the ones most likely to detect fraudulent behavior. Co-workers must be willing and encouraged to repeat experiments as well as to engage in critical discussion of all laboratory procedures, data and reports. Frequent conferences should be held that include all laboratory personnel for the

purpose of critically evaluating all the experimental data collected. This reduces the possibility of data falsification by any member of the laboratory and increases the possibility that all participants will better understand the work being performed. Reporting fraudulent activity is the responsibility of all laboratory personnel.

Professional peers are likely to detect fraudulent activity as they critically review the experimental work being reported at various stages of a research project. Critical reviews of research proposals and the work being submitted for publication can lead to the detection of fraudulent activity. This mechanism has certainly been effective in detecting plagiarism. Peers who assume the responsibility to review research proposals or manuscripts for publication must seriously assume that responsibility and critically examine all experimental results for the possibility of aberrant results and for possible fraudulent reporting. The exercise of such a responsibility is the basis for continued self-regulation.

Review committees involved in the academic personnel process are also in a position to detect fraudulent activity if they exercise their responsibility for critically evaluating the research included in the material under review. Multiple reporting of the same data and a large number of publications using multiple authorship require special consideration. Review of the research material presented for evaluation should be carried out routinely by professional peers outside the institution.

Program officers and review committees assigned to evaluate research proposals submitted to federal granting agencies are also positioned to detect fraudulent behavior. The peer review system used by federal agencies is very extensive and provides a unique opportunity to critically evaluate preliminary data, and to question the claims made on the basis of such data.

How do, or should, Academic Institutions Inquire into Allegations of Fraud?

The following are presented as guidelines for inquiring into allegations of misconduct. Many are adapted from the University of California Policy on Faculty Conduct and the Administration of Discipline (1974). Others are included in the recent report entitled, "Maintenance of High Ethical Standards in the Conduct of Scientific Research," issued by the Association of American Medical Colleges (1982).

Reporting seeming violations of intellectual honesty is a widely shared responsibility and the procedures for doing so should be understood by the entire community. The research environment allows the opportunity for a variety of individuals to observe possible misconduct, including students, laboratory associates, professional peers, departmental colleagues and administrative officers. Each shares the responsibility for reporting misconduct and each institution has the responsibility of providing an environment that does not discourage such reporting. Reports of alleged violations must be taken seriously with the recognition of the need to protect the rights of both the accused and the accuser.

Investigating alleged violations must be carried out using established procedures that are well understood by the university community. Suspected misconduct should be brought to the attention of the Project Leader, Department Chair, Dean or President depending on the circumstances. An appropriate extra departmental committee, standing or ad hoc, should be assigned the responsibility for making a preliminary investigation and recommending a course of action. Investigations should be carried out in a timely manner and assure confidentiality. Faculty and peers should participate in the investigation and in the procedures that determine the administration of discipline. Procedures should be available for the possible disposition of cases of alleged fraud at the lowest appropriate administrative level. This could be at the level of the project leader. However, a process must also be available in the event resolution does not occur at these lower administrative levels. A written record of all investigative findings must be maintained. All persons collaborating with the individual being investigated should be informed confidentially of the allegations. Such persons include research collaborators and administrative officials responsible for managing any contracts or grant funds used to support the program in question. Institutions should identify an office that is responsible for coordinating inquiries of alleged misconduct and information regarding the functions of that office should be made widely known to the community.

Procedures for investigating alleged misconduct must provide the opportunity for the accused to be heard. Investigatory procedures must allow for the review committees, whether an ad hoc committee specifically appointed to review the case or a standing committee on privilege and tenure, to conduct hearings and to provide the accused an opportunity to present his or her testimony. The accused should also be advised of the decisions of the committee and the extent of dissemination of the information related to that decision.

Disposition of cases involving alleged fraud must not be made until a thorough review of all the relevant research activities involving the accused is completed. Investigatory procedures for disposing a case of alleged fraud must not be terminated until a full investigation of all facets of the case has taken place. Even the perception of a perfunctory investigation must be avoided. Without a full and complete investigation, additional reports of misconduct not considered in the original disposition of the case would carry the appearance of stone-walling. Early negotiated settlements of cases of alleged fraud are likely to avoid a thorough review. For this reason, investigations must be assiduously carried to completion regardless of the procedures used.

Discipline must be in accordance with guidelines established with the concurrence of the faculty. Sanctions appropriate to violations of research integrity must be fairly determined from existing guidelines related to misconduct developed with the concurrence of the faculty. Generally approved sanctions applied in cases of misconduct include warning, written censure, demotion, suspension and dismissal. The type of sanction applied must be determined on the basis of the evidence presented in each case and the severity of the aberration as determined by those reviewing the case.

Violations of research integrity must be communicated immediately to all individuals and agencies affected by the misconduct. All individuals and agencies involved with the researcher found guilty of fraudulent activity must be informed of the findings. Such individuals and agencies include research collaborators, editors of scientific journals (if the work is published or under review for publication), granting agencies (if the work is funded), and institutional officers responsible for the personnel process or the administration of relevant contracts or grants. Although granting agencies must be informed of misconduct related to their programs, it is not clear at what point in the investigation they should be informed (Broad 1982; Sun 1982). Every effort must be made to protect innocent collaborators. The findings should be incorporated into the individual's personnel file.

Many universities already have general statements regarding faculty misconduct and some have well established policies and procedures that incorporate a code of conduct. Such codes of conduct generally recognize both the rights and responsibilities of the faculty. The professional rights of the faculty include the expectation of an environment protective of free inquiry and of judgments regarding their professional activities being fairly made by peers. The professional responsibilities of the faculty include the obligations of self-discipline and intellectual honesty. It is important that procedures used for inquiring into alleged misconduct recognize these rights and responsibilities.

What can We do to Prevent Fraud?

The primary responsibility for the prevention of fraud in science rests with those who do science. It is the special responsibility of principal investigators who serve as mentors within the laboratory (Lodish 1982). The responsibility cannot be readily assumed by administrators, journal editors or granting agencies. They are too far removed. On the other hand, the principal investigator is in a position to control the environment of the research laboratory, to insist on the validity of all data collected and to provide the standards for critical evaluation of all ideas and writings emanating from the laboratory. They are the ones who must insist on self-criticism of all who work in the laboratory. To carry out their responsibilities, they must remain close to all the work in the laboratory.

For instance, the blue-ribbon committee, appointed to review the fraudulent activity recently disclosed at Harvard University, opined about the question of how to prevent fraud in science. The measures proposed by them were included in the article entitled, "Prevention of Dishonesty in Science," that appeared in Science (1982). These measures were: (1) Special attention should be given at the time of recruiting to the motivation and integrity of the applicant through careful examination of credentials and claimed accomplishments; (2) Written, detailed, explicit procedures for data gathering, storage, and analysis are essential and should be available in all laboratories; (3) Fellows should be supervised by experienced scientists in the laboratory, including regular in-depth scrutiny of the primary data and the calculations leading to the presentation of results. The laboratory director should, by example, develop in the fellows a respect for primary data and its preservation; (4) The conduct of studies which are blind or coded, and the exchange of reagents or methods between laboratories should be encouraged. The repetition of research in the same laboratory and by different research teams is desirable whenever possible; (5) Fellows should be encouraged to work with other colleagues, to share data and to engage in free discussion of results. Secrecy about methods and data should be discouraged; (6) Emphasis should be placed upon the quality and significance of research rather than on quantity and visibility. The laboratory director should accept responsibility for the quality of the work reported from the laboratory and should actively discourage the submission of multiple abstracts to national meetings. Abstracts should be considered as reports of results worthy of publication and as forerunners of complete papers rather than as bids for appearance on a national program; and (7) There should be close personal interaction between faculty and fellows. Among the many benefits of such close interaction could be the early detection of personal problems or unusual personality traits.

Similar measures are contained in the recent report issued by the American Association of Medical Colleges (1982). I do not believe that I can add to this list. As a scientist and administrator, however, I do not believe that I can subtract from the list. It is important that institutions encourage all measures for the prevention of fraud and treat fraudulent activity very forcefully when it occurs. No less important, the rights of principal investigators to continue using university facilities, to be allowed to exercise their responsibility of teaching, to apply for extramural financial support for their work, and to otherwise share in the rewards of being faculty must be protected only as long as they are deserved.

SUMMARY AND CONCLUSION

Cases of fraud in science, along with discussions of the possible causes and consequences of fraud, are appearing in an increasing number of publications and meetings. This paper is narrowly, but purposely, focused on the issue of fraud and includes discussions of its character, detection, investigation and prevention.

Fraud in science continues to be unacceptable. The increased attention being paid to cases of fraud, however, has raised concerns about the environment of scientific research and its relationship to fraudulent activity (e.g., increased reliance on large research teams, greater competition for scarce resources, and higher potential for personal financial rewards in certain areas of industrial interest). These concerns become heightened when conflict of interest is introduced as an element in the activity.

The potential for fraudulent activity must be taken seriously by all who practice and administer research and must be dealt with expeditiously and forcefully by colleagues, peers and administrators. Research integrity will be assured only when the recognition of its importance is appreciated and the responsibility for its protection is fully shared by all who do research. The privilege of continued self-regulation depends on the recognition of that responsibility just as the progress of science depends on the recognition of self-regulation.

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COMPLIANCE AND NONCOMPLIANCE WITH FEDERAL REGULATIONS FOR THE PROTECTION OF HUMAN SUBJECTS

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Thirty-six years ago the Nuremberg Tribunal issued a declaration of the rights of human subjects involved in research. Since that time biomedical and behavioral research has expanded and accomplished goals that equal and transcend all previous accomplishments in the life sciences. The research community can take justifiable pride in its scientific achievements. By the same token it can take justifiable pride in the fact that it has developed ethical principles, practices and procedures which constitute an extraordinary framework of protections for the dignity and the autonomy of human subjects as well as assuring the beneficence of the research enterprise.

There can be no question that research involving human subjects has produced marvelous benefits for society, including the prevention, diagnosis, treatment and cure of illness and the relief of suffering. In the United States the partnership between the public, which endorses and supports the major portion of biomedical and behavioral research, and the scientific community, which carries research forward in a fashion which is both free and accountable, is unsurpassed in human history.

Appreciation of this background is an appropriate starting point for addressing questions relating both to misconduct in science and public constraints on the conduct of science. The primary goal of biomedical research is to control or prevent disease and promote health in human beings, as well as other vertebrate animals, and to improve the quality of life. One aspect of the quality of life that we seek is to assure that we live in a society that respects the rights and welfare of every human being, particularly those involved in research. If we are to build upon our tradition of excellence, we must strive to understand and to implement policies that provide for the protection of human research subjects with the same diligence that we employ in constructing and carrying out the research designs.

In the past few years an increased number of allegations of misconduct in science have come to the attention of the National Institutes of Health (NIH), the world's leading supporter of biomedical research, and an agency that shares responsibility with the research community for protecting human subjects. It is startling to realize that in the past two years NIH has investigated more instances of alleged misconduct than was true in the entire previous decade. Even if all of the allegations could be confirmed (which, of course, they cannot), their absolute number would constitute only a tiny fraction of all of the research conducted in this country. Nevertheless, because some allegations of misconduct have proved to be true and because the associated publicity has been widespread, the integrity of the entire research community has been placed in a shadow.[1] Public confidence in the entire research endeavor has been shaken by the dramatic breach of faith of a few careless or misdirected individuals.

The public has become painfully aware of falsification, fabrication, intentional misrepresentation or mishandling of research data. Sad to say, we have become aware of investigators who have failed to comply with the terms and conditions of a research grant or contract. We know of instances of the misuse and misapplication of funds. And finally, we have learned of noncompliance with the regulations for the protection of human research subjects and violations of the Public Health Service.

Policy for the Care and Use of Laboratory Animals

NIH Policies and Procedures Concerning Scientific Misconduct

In response to such cases the NIH has begun to develop policies and procedures related to real or apparent misconduct. Included under the rubric of misconduct are such matters as: (1) the mismanagement of funds, such as inappropriate rebudgeting or making unauthorized expenditures; (2) fraudulent or grossly irregular practices in carrying out research procedures or reporting research data and results; (3) serious failures to comply with the Department of Health and Human Services (HHS) regulations governing the protection of human subjects or Public Health Service policies for the care and use of laboratory animals; and (4) serious failures to comply with other conditions of an award, such as conduct of research that departed significantly from that which was approved by the funding agency.

The purpose of the new procedures is to ensure, as far as possible, that the NIH actions with respect to allegations of misconduct will serve to protect the public interest and provide fair treatment for the accused. The task is far from easy. On the one hand, one must never condone unlawful or unethical behavior on the part of scientists or research institutions; on the other hand, one must keep in perspective that the number of offenses is small and the remedy for these offenses must not be so severe as to undermine or jeopardize the entire research enterprise.

The NIH effort to date has concentrated on several areas: first, the detection of real or apparent problems; second, an inquiry as to whether a real problem truly exists; third, procedures for the investigation of problems; fourth, the imposition when warranted of temporary sanctions prior to completion of an investigation; and finally, administrative action following completion of an investigation.

In each of these areas the NIH is traveling in unfamiliar territory because new procedures are being developed and roles of operating offices defined. The NIH has traditionally worked hard to establish a

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[1] This paper avoids specific statements about these allegations because of sensitive administrative and legal matters still under investigation.

trust relationship between the funding agency and awardee institutions. In developing new procedures, the NIH must be careful not to taint that fundamental relationship of trust which is essential to research in the life sciences in this country.

The Office for Protection from Research Risks

While the NIH must deal with a broad spectrum of unethical conduct in science, my experience has been confined almost exclusively to checking allegations of improper conduct in research involving human research subjects or misuse of laboratory animals. Consequently, I shall not attempt to address the broader issues of misconduct but will confine my comments to the area of research involving human subjects.

Over the past several years the Office for Protection from Research Risks (OPRR) has been involved in the investigation of approximately 25 cases of alleged noncompliance with human subjects' regulations (for example, in the areas of absence of or use of inappropriate informed consent documents; failure to obtain review and approval by Institutional Review Boards (IRBs); conduct of research with human subjects not approved by the funding agency; and falsification of research data). Although several of these cases have received widespread publicity, most of them have received little publicity outside my office and outside the institutions where the research was conducted. Often after we carried out a thorough investigation of the situation, we found that there was no basis in fact for the allegations brought against certain institutions and individuals. This is an important consideration since we were able to minimize publicity in these cases, and to resolve the issue with little or no damage to the reputations of innocent persons. Of course, this has not always been, nor will it always be, possible.

Misconduct in science is newsworthy. It is newsworthy because it is relatively rare. Let us hope that it always remains so.

Investigating Allegations of Noncompliance with Policy to Protect Human Subjects

Let me describe for you a typical sequence of events that occurs over a period of weeks or months when an allegation of noncompliance comes to our attention.

First, the possibility of an infraction may come to our attention by many routes: (a) the NIH program or review staff may identify a problem with data or a deviation from an approved course of action; (b) an IRB may become aware of possible noncompliance with HHS regulations or institutional policy, as set forth in the assurance of compliance, and notify us through institutional officials; (c) a colleague, student, nurse or subject involved in the research may inform us directly or notify us indirectly through institutional officials; (d) anonymous individuals may present us with allegations of noncompliance; and finally, (e) the news media may publish allegations of noncompliance.

Second, when an allegation is received a preliminary inquiry takes place: (a) the research application or applications of the investigator are reviewed to enable us to understand what research is supposed to be ongoing; (b) annual reports are scrutinized to bring us up to date on research activities; (c) other applications in the same research unit are reviewed so that we may understand the context in which the questioned research is being conducted; (d) the institutional official identified in the institution's assurance of compliance[2] is contacted and asked to examine the matter and report back to OPRR

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[2] HHS regulations for the protection of human subjects require each institution engaged in research covered by these regulations to provide written assurance to the Secretary, HHS, that it will comply with the regulations. Only if the institution has an approved assurance can it conduct the research funded by the HHS. At a minimum the assurance must contain

> (1) A statement of principles governing the institution in the discharge of its responsibilities for protecting the rights and welfare of human subjects of research conducted at or sponsored by the institution, regardless of source of funding. This may include an appropriate existing code, declaration, or statement of ethical principles, or a statement formulated by the institution itself. This requirement does not preempt provisions of these regulations applicable to Department-funded research and is not applicable to any research in an exempt category listed in Sec. 46.101.
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> (2) Designation of one or more IRBs established in accordance with the requirements of this subpart, and for which provisions are made for meeting space and sufficient staff to support the IRB's review and recordkeeping duties.
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> (3) A list of the IRB members identified by name; earned degrees; representative capacity; indications of experience such as board certifications, licenses, etc., sufficient to describe each member's chief anticipated contributions to IRB deliberations; and any employment or other relationship between each member and the institution; for example: full-time employee, part-time employee, member of governing panel or board, stock-holder, paid or unpaid consultant. Changes in IRB membership shall be reported to the Secretary [OPRR].
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> (4) Written procedures which the IRB will follow (i) for conducting its initial and continuing review of research and for reporting its findings and actions to the investigator and the institution; (ii) for determining which projects require review more often than annually and

by a fixed date concerning the alleged noncompliance; and (e) the research investigator or other individual who may be involved receives a copy of our letter to the institutional official so that he or she knows precisely what is charged and who has responsibility for the inquiry.

I am pleased to report that in many cases OPRR receives a prompt response to its inquiry which provides clear documentary evidence that the allegation cannot be supported. In situations of this kind no further steps are taken, and a note is placed in the file indicating that the allegations have been evaluated and dismissed. We try to maintain confidentiality in these proceedings. In most instances we are successful.

Third, if the initial inquiry suggests that inappropriate activity has occurred or leaves the matter in doubt, then additional steps are taken. The institution may be asked to carry out an inquiry to determine the facts. The institution may utilize its IRB, another standing committee or an *ad hoc* body to conduct its investigation. Alternatively OPRR may, in cooperation with the funding institute, conduct its own investigation or site visit to determine the facts of the matter.

Fourth, the accused individual or institution is included in the NIH "alert" system so that if additional funds are sought, the Director's Office will be notified.[3] The alert system is not a penalty or a prejudgment. It simply makes senior NIH officials aware that an individual or an institution under investigation has applied for funding. At this stage decisions may also be made whether to notify the Department of Justice and the Office of Inspector General.

Fifth, when all available information has been gathered, including statements from all interested parties, OPRR prepares a report on the matter and forwards it to the Associate Director for Extramural

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which projects need verification from sources other than the investigators that no material changes have occurred since previous IRB review; (iii) for insuring prompt reporting to the IRB of proposed changes in a research activity, and for insuring that changes in approved research, during the period for which IRB approval has already been given, may not be initiated without IRB review and approval except where necessary to eliminate apparent immediate hazards to the subject; and (iv) for insuring prompt reporting to the IRB and to the Secretary [OPRR] of unanticipated problems involving risks to subjects or others." [45 CFR 46.103(b) (1)-(4)]

[3] The NIH Alert is a system internal to the agency for collecting, controlling, and disbursing information to appropriate persons to facilitate their making informed decisions regarding award of grants, cooperative agreements or contracts which involve organizations and/or persons charged with or found culpable of unacceptable behavior.

Research and Training at NIH. (The report contains recommended courses of action.) The Associate Director then establishes a committee to review the report. Usually the committee consists of approximately five directors or senior officials of the various institutes of NIH, including the director of the funding institute. OPRR staff in my office are asked to defend their findings. Often the report is clarified and modified. When it meets the committee's approval it is forwarded to the institution in which the accused investigator has conducted research and to any implicated research investigator for comment. Comments received are attached to the report.

Sixth, the report with the attached comments is reviewed once again by the NIH committee composed of the institute directors or senior officials. The report may be modified in the light of the comments received. The committee then recommends sanctions if they are appropriate. When this modification is completed, the report is forwarded to the Director, NIH, for final decision. The Director may, when appropriate, consult with the Assistant Secretary for Health or with the Secretary, HHS.

CONCLUSION

In conclusion, noncompliance with human subjects or animal welfare policies regulations may be harmful, directly and indirectly, to the researcher, the research institution, the research community in general, the public, as well as to those human subjects involved. Although cases of misconduct are relatively small in number, their implications are vast for public trust and continued academic freedom.

At their present stage of development, the policies and procedures of the NIH described in this paper, and the inevitable modifications of them to fit particular situations, have not been published. We hope to publish them soon for comment. They have evolved in response to a small number of cases. No doubt they will continue to evolve.

As you can see, the HHS regulations place primary responsibility on the research institution to comply with regulations and to investigate possible noncompliance. We hope to keep Federal "policing" or "monitoring" procedures to a minimum. OPRR stands ready to cooperate with and support institutional research administrators in every reasonable fashion. It is our intention to work with research institutions to protect and defend, so far as possible, the integrity and credibility of the research enterprise. With your cooperation we believe we can do so.

Our experience has shown that research administrators are willing to assume the responsibility to assure that research is being conducted properly. The Association of American Medical Colleges (1982) has developed its own guidelines for ensuring the propriety of the conduct of research sponsored at the institutions, and wherein rests the major responsibility for prevention of misconduct in science. The American Association of Universities is currently preparing a similar document.

So long as we enjoy the cooperation of research administrators and the support of leading professional associations, we have reason for confidence in dealing with the unpleasant but necessary responsibilities arising from misconduct in science.

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THE LEGAL BASIS FOR FEDERAL PROSECUTION OF SCIENCE MISREPRESENTATION AS "WHITE COAT" CRIME

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Since 1975, this author has advocated that institutions and their scientist employees be prosecuted under federal criminal statutes for knowingly publishing falsified scientific data. This prolonged advocacy, which arose initially from personal contact with one case, has raised questions about the objectivity of the author. The apparent nonresponsiveness of the National Institutes of Health has raised doubts as to the degree of seriousness of the problem.

The idea that the educational elite should be treated equally under the law does not arise from the mere disgruntlement of a single employee with the conduct of a single institution. Continuing journalistic exposés of scientific plagiarism, falsification of data in publications, drug testing and federal grant applications, and the "puffing" of investigator productivity and qualifications by several of the educational elite provide unequivocal evidence of the failure of past laissez faire policies. Such reports also vindicate the position of those advocating a vigorous enforcement policy of existing federal law.

Following the oral presentation of this paper, I became keenly aware of two issues in need of discussion. First, since the scientific community has heretofore considered misconduct merely unethical, there is some uncertainty as to the identity of the public's interest to be protected or remedied by the invocation of the criminal law sanctions. Second, there is a gap in the understanding of those in the legal profession and other laypersons with the authority and day-to-day functioning of the National Institutes of Health.

My analysis indicates that scientific fraud may be considered a criminal act where the misrepresented information is knowingly and willfully published and used in the federal grant or contracting process. It should be understood that under the statute to be analyzed, the prohibited conduct is not that as between private parties, such as homicide, embezzlement or theft. The prohibited conduct contemplated here injures the Government itself as it exercises a congressionally mandated, authorized function. Specifically, the agency is the National Institutes of Health, the authorized function is the distribution of public funds in support of extramural scientific research, and the prohibited conduct is falsifying information in the course of obtaining or using those funds.

Due to editorial constraints, it is not possible to completely delineate the authority of the National Institutes of Health. But I will submit the following brief explication: The NIH is not primarily a regulatory agency in the traditional governmental sense, such as the Interstate Commerce Commission or the Nuclear Regulatory Commission, and although certain safety and human subjects protection regulations have been recently promulgated, the agency has little authority to regulate the day-to-day conduct of scientific experiments. However, the NIH does "regulate" the course of scientific research in a most profound way by its funding decisions. By law, these funding decisions are based upon expert evaluation of investigator initiated applications for funds. Therefore, the essence of the ability of the NIH to regulate the quality of scientific information lies in its authority to police the truthfulness of the claims made to it in the course of such applications. The general proscription against falsifying information is to be found in Title 18 of the United States Code section 1001.

The purpose of the present article is to explore the nature of the injury(s) of scientific misrepresentation, and to briefly explain the pivotal role of the NIH in the enforcement of the criminal code, although it is merely a funding agency. This article also reviews federal court decisions construing the statute proscribing the submission of false data and statements to any government agency and relates those decisions to the falsification of scientific data.

The Protectible Public Interest

Cheating in science does not present an ethical dilemma (Woolf 1981). There is clear agreement that it is wrong to falsify data. Yet, it is a mistake simply to infer from the statement that it is unethical to cheat, that these are mere violations of rules drawn up by members of a private group, or that the nature of the remedy lies in the corrective measures designed by the members of the private group. Such a position overlooks the strong public involvement and interest in the reliability of scientific information.

Science in academia is a 3.5 billion dollar a year government business, which is primarily monitored by academic scientists. The self-monitoring of science has arisen, in large part, out of a great faith in the disinterestedness of scientists, as reflected as long ago as 1906 with the passage of the Pure Food and Drug Act which stated, in part, that whether substances were deleterious was to be left to the Secretary to decide "under the guidance of proper disinterested scientific authorities," and after most careful research. Nevertheless, some believe (Bazelon 1979) this faith in the disinterestedness of scientists is naive. Concern centers upon the limits of the scientist's ability to make policy judgments, rather than the integrity of the process of determining scientific fact. There is further recognition that scientists are steered by such self-interests as status consciousness, the personal exercise of authority, the availability of economic gain through public and private consultation and the desire to engage in local empire building.

If science were a private calling, an essentially privately supported enterprise, then one might argue that the public has no legitimate interest in the scientist's private foibles or in the way scientists organize their institutions and activities. With respect to public funding, however, it is clear that the administrators of federal funds (Frederickson 1981) have both legal and fiduciary duties to insure that the federal dollar is spent wisely and according to public policy dictates. This obligation is primarily due to the following recognition: "(f)or as long as governments have operated benefit programs to combat social problems, some people have sought, and often found, ways to divert program funds for their own benefit. Frauds can range from a few dollars to millions of dollars, can involve simple misrepresentations or complex patterns of concealment, and can occur almost anywhere" (Gardiner, Hentzell and Lyman 1982:1).

While the magnitude of the financial fraud problem in government is uncertain, Gardiner et al. (1982) report: (1) in 1978, in relation to the Aid to Families with Dependent Children program, about $265 million was estimated as being lost to fraud and abuse; (2) former HEW Secretary Califano estimated that fraud in the Medicaid program totaled $750 million per year; and (3) in five states, the GAO found that at least $222.6 million in substantiated or potential Medicaid overpayments (not all representing fraud) had been identified. Gardiner et al. (1982) also cite examples in the health services area as evidence of some doctors, nursing homes, and pharmacies billing the government for unnecessary, duplicate and/or non-rendered services, and even taking steps to conceal such improper conduct.

Some scientists have been convicted for similar violations of federal criminal statutes. This is evidenced in two cases of filing false travel reports for reimbursement (Privilege and Tenure Committee Report, University of Colorado 1978; Broad 1980) and one case of lying in a conspiracy to traffic in regulated drugs (Smith 1980). A common feature in the above incidents involving scientists and academics was that the criminal convictions resulted not because the scientists in question had lied about the subject matter of their profession (i.e., their research), but because they had obtained improper personal benefits through "diversion" of the federal funds earmarked for specific use.

Yet, in contrast to the diligence with which government-related cases involving diversions of funds have been generally prosecuted, there has been a "singular" lack of investigation and prosecution in the area of science misrepresentation. Continuing reports of fraudulent scientific misrepresentation indicate that the lack of objectivity extends to the fact gathering process as well. In spite of this lax attitude, it is my contention that the public has an interest in protecting its investment in the science enterprise, whether it be in relation to those cases involving the diversion of funds or the perversion of the fact gathering process.

The Nature of the Injuries

If one were to rank diversion of funds and falsification of data in terms of the seriousness of consequences, the more serious consequences would follow from publishing falsely, since the result is additive. Not only are scarce resources squandered in the search for new knowledge, but false information is also introduced into the body of scientific literature. Furthermore, such injurious effects may not be ascertained for prolonged periods of time. We are not concerned, of course, with the unusual situation of non-publication of a fake experiment since that act, if one were to reflect a bit upon it, can only deceive the experimenter, if there is any deception at all. Rather, for there to be a potential injury, the information, claim or citation must be made to another party.

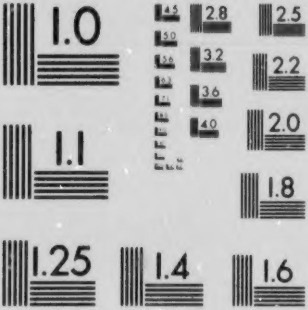
The seriousness of the effects of misrepresentation may be measured in terms of the institutional and individual costs involved. Some researchers will rely on such publications as valid sources of information and will be led off on a fruitless line of inquiry. There will be others who will attempt to replicate the experiments and discover the results are not reproducible. This latter consequence may result in the abandonment of the line of inquiry and/or in the wasteful expenditure of funds, time and talent in an attempt to determine why there was a failure to replicate. Woolf (1981) has recently testified that an estimated total of $550,000 and six months of effort of seven different research groups were spent in following up just one episode of specious research. Also, the careers of co-workers were threatened by the time lost and the guilt by association with the guilty party.

Improper personal benefits arise where falsifications contribute to successful publication. Long (1981) testified that after one of his papers was rejected, rather than repeating a questioned point, he falsified the results to satisfy the critique. The paper was accepted after resubmission. False data may thus be represented as work which has passed stringent peer review and used to obtain further funding, salary increases, and to gain unfair advantage over other academic scientists.

Scientific fraud also has a most profound negative effect upon the educational mission of academia. Government officials (Fredrickson 1981) explicitly argue that their rationale in providing public funds to academia, in the form of training funds, is to insure that it is a place where the scientific method is taught and practiced, and where the "internal ethic" of science is maintained and passed along from generation to generation as an intellectual discipline and a critical faculty. Clearly, if it is the example of misconduct that is being passed along, then there is a most significant injury to the public's interest.

Further, scientific misrepresentation exacts its toll on the scientists who seek to protect the integrity of their profession and in the course of such conduct face harassment, discharge, or litigation (Capron 1981). Scientific misrepresentation also diminishes the confidence of the public in the ability of science to govern itself, and thus allowing the less enlightened elements to wield the axe instead of the scalpel. To paraphrase the Tenure and Promotion Committee of the University of Colorado (1978):

> Research activity in the United States depends ultimately upon the good faith of the individual researcher. A system of meticulous oversight imposed on the science establishment is the ultimate threat to the freedom of scientists. Filing truthful documents and publications is the least demanding task for academic professionals and is an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MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS
STANDARD REFERENCE MATERIAL 1010a
(ANSI and ISO TEST CHART No. 2)

> indispensable element in the preservation of that freedom. (p. 13)

Finally, the NIH has asserted, and it is undoubtedly true, that there are more deserving and worthwhile research projects than there is available funding. Ostensibly, there have been potentially productive scientists who were not funded solely as a result of their comparatively lower ranking. These scientists may have been displaced by those who, but for their false claims of quality work, would not have otherwise been funded. Thus, there is the loss in productivity from talented people with integrity and of reliable information. A loss that effects both the scientific community and the American public.

National Institutes of Health Policies in the Control of Science Fraud

The Function of the National Institutes of Health

The question has been raised as to how the National Institutes of Health, basically a funding agency, plays its role in the prevention of future transgressions and in the remedying of past ones through prosecution of the criminal code. The procedure by which the NIH accomplishes its mandated purpose is unique among federal agencies. Understanding these procedures is central to understanding those circumstances whereby scientific fraud may be considered a federal crime. In addition, the guidelines for dispersing funds provide the framework for understanding how the NIH should proceed upon receipt of allegations of scientific fraud.

The NIH is primarily a funding agency within the Department of Health and Human Services. Grants are made under the authority of 42 U.S.C. 241(c). The principal purpose of the grant mechanism is to "accomplish a public purpose of support or stimulation authorized by Federal statute, rather than acquisition, by purchase, lease, or barter, of property or services for the direct benefit or use of the Federal Government..." (41 U.S.C. 504) (Federal Grant and Cooperative Agreement Act, Section 5[1]).

The initiation of applications for research support arises outside the government agency from the investigators themselves. In order to assure competent evaluation of each proposal, a system of "peer review" has been established. The peer review system relies upon panels of nongovernmental consultants who are organized around particular specialized disciplines. These panels, called "initial review groups" (IRGs), consist of from ten to twenty members, only one of whom, the Executive Secretary, is an NIH employee. The policy considerations behind this arrangement are twofold: First, nongovernment consultants are generally active in the field of expertise while the government members are, or have been for some time, administrators; second, it lessens the potential of direct interference with the conduct of scientific research by political appointees.

Applications are referred to members of the IRG who undertake to evaluate each application, gathering such additional information as may be necessary to that task. This may involve a "site visit" to the facility at which the applicant proposes to conduct their research. The applicant and their institutional representatives typically present oral and written statements; data in defense of their proposal are presented in response to questions arising from experts who have studied the materials in the application. An evaluation of each application is written by the assignee group and circulated to the whole IRG prior to a meeting during which the application is discussed at length and a recommendation voted. If approval is recommended, it is given a relative priority rating. A summary statement is then made which describes the proposal and recounts the substantive considerations that led the IRG to recommend approval or disapproval. It contains an opinion of the professional qualifications of the sponsor and an evaluation of his competence and facilities.

At this point each application and its Summary Statement is submitted to an Advisory Council, consisting of various administrative officials and private citizens. This Council advises and recommends actions to the Secretary of the Department. About 90% of approved applications receive funding. Often nobody other than the IRG will have examined the scientific merit of a particular application. Also, Federal regulations governing supervision of grantees allow for the review of periodic reports submitted by grantees, on-site visits, and require agency approval of major program or budgetary changes (45 CFR).

NIH's Associate Director for extramural research, Dr. William Raub (1981), and the then Director of NIH, Dr. Donald Fredrickson (1981), have both testified to Congress concerning the review criteria for funding these applications: namely, the scope of the scientific and technical assessment includes not only the originality of the research problem and soundness of the proposed approach, but also the investigator's credentials, including publication record. The reliance upon publication record is based upon the recognition that prepublication review, especially for the more prestigious journals, involves formal and well-documented critiques by recognized experts.

There is one additional consideration which attempts to protect the objective nature of the decision-making process for funding scientific research. Under the terms of the conflict of interests statute and regulations of the NIH, an individual who is related by blood, marriage, or employment to the institution or individual who has a proposed study may not participate in the discussion or decision of such study. The individual may not be present in the room when such discussion is being carried out.

Discerning NIH Policies in Fraud Control

The continuing reports of science misrepresentation in the lay and scientific press argue for the proposition that past NIH policies have failed to protect the public's interest in disbursing public funds fairly and wisely and in assuring that alleged scientific observations were in fact made. In my opinion, a major contributing factor to this situation is that the NIH has not constructed an analytical framework against which to develop sound policy to control scientific fraud. I have previously recounted several private and public interests that are injured in the course of the perpetration of scientific fraud. In order to have a policy which protects the government's interests, I have outlined what the functions of the NIH are and how those functions are accomplished.

To summarize, the function of NIH is to provide a mechanism by which public funds are distributed for the purpose of supporting extramural scientific research in academic institutions and not in profit-making private institutions. The distribution of funds is based upon a formal merit system which depends on competitive evaluation by appointed non-government experts in the area of the proposed field of study. Such evaluations are made after studying the voluntarily written and oral statements of applicants that describe their proposed study, their qualifications, and their past productivity. The NIH has essentially no authority in relation to the day-to-day con-duct of scientific research (nor does the author propose any such in-vasion), although it does have limited authority to issue and enforce regulations for certain NIH supported programs involving human sub-jects engaged in potentially dangerous experiments.

Therefore, the government's direct interest in assuring high quality research from its funds is advanced almost solely through its funding decisions, which are based upon the technical evaluations of the applicants. It is self evident that the submission of false statements and the use of fictitious data in applications and reports corrupts the integrity of an authorized function of the NIH; it does so by deceiving the experts employed by NIH to evaluate the merits of the proposal and the qualifications of the investigator(s). I submit, then, that the policing of the truthfulness of claims made to the NIH is a primary and non-delegable-duty of the NIH and should be the framework around which its policies must be formulated.

The question now arises as to the specific features of whatever policy the NIH follows when called upon to investigate allegations of science fraud. The most recent statement of this policy was given to Congress by NIH's Associate Director William Raub, Ph.D. (1981). According to Raub, the policy "has been to treat these on a case-by-case basis" (p. 176). He went on to say that the people working on the cases were in a small unit with a "background in the area of audit function" (p. 176). Clearly, this statement is not illuminating, although it would be a serious error to assume the absence of policy or that the policy pursued is not discoverable. The policy pursued may be discerned by observing NIH's pattern of conduct vis á vis the synthesis of documents now obtainable through the Freedom of Information Act. These records indicate that NIH's conduct is not haphazard, as Dr. Raub would have one believe.

Using the Freedom of Information Act, I was able to obtain records with respect to NIH investigations of four principal medical research institutions in the United States: Yale University, Massachusetts General Hospital, Stanford University, and the early records of the Marc Straus investigation at Boston University. (Those interested in the primary documents need only make a written request to NIH's Freedom of

Information office in Bethesda, MD.) One position taken by the NIH in response to allegations of scientific fraud in publically funded research is to assert that the matter is an internal or scientific dispute. From this position, NIH claims to have no duty or responsibility in these matters. For example, in December 1975, this author (Dong 1975) presented evidence of scientific fakery in an NIH application for funds to the attention of the then Assistant Secretary for Health, Dr. Theodore Cooper. The documents supporting the above allegation were obtained in the course of a Stanford University faculty investigation of one Zoltan J. Lucas, M.D., against whom charges of scientific fraud had been brought by one A. Baronio Martins, Ph.D. Aspects of that dispute eventually reached the California state civil court and will be referred to hereinafter as Martins v. Lucas et al. Ronald Lamont-Havers, M.D. (1981), the then deputy director of NIH, stated briefly in response to the author's letter that the NIH would not be a referee in this scientific matter, and that challenges should be taken up through "normal scientific discussion." On March 25, 1976, this author raised the question to NIH as to what scientific channel was to be used when there was a report of misrepresentation in a NIH document. There has been no reply to that question. The NIH closed its file on the matter (Jacobs 1976).

This same policy stance was articulated by the then Director of the National Cancer Institute (NCI), Dr. Arthur Upton, in the more highly publicized case of Dr. Marc Straus and Boston University. Boston University officials had already obtained the information that patient-related records had been falsified and that Dr. Straus had resigned. They told NCI officials of the submission of falsified data to the Eastern Cooperative Oncoloy Group (ECOG), a NIH funded multi-institutional cancer research project. NCI, wrote Dr. Upton, could not "intervene in the internal affairs of institutions" (Upton 1976:1) in matters in which the NCI was not directly involved.

However, this policy of non-interference does appear to change if allegations of misrepresentation reach Congress or the mass media. One month following a five-part Boston Globe expose of NIH's conduct in the Marc Straus case, the NIH announced that "criminal" laws may have been violated and initiated an investigation (Bruzelius 1980). Following a request for a report on the Stanford case by Senator Edward Kennedy in 1977, the NIH thought it appropriate to reopen its investigation of the matter.

When thus pressed, the NIH falls back to a second position, the logic of which is ultimately faulty. In a letter to Senator Kennedy, Dr. Donald Fredrickson (1977) first enunciated the position that it is the policy of NIH to rely on the integrity of an institution to inform the NIH of wrongdoing of a university's employees. This position is derived from the fact, or so Dr. Fredrickson asserted, that the university is the legal entity and recipient of the award of public funds. This statement was made directly after acknowledging that Stanford University acknowledged it had not informed the NIH of wrongdoing on the part of one of its employees and had not disclosed the results of its faculty committee's investigation. It is the policy of the NIH to accept even a telephone denial as a report of an investigation of scientific fraud from a client institution.

This point is sufficiently important to directly quote from the subsequent letter:

> On May 26, 1976, Mr. Schriver met with Mr. John Schwartz, Legal Counsel, Stanford University Medical Center. Mr. Schwartz reviewed the actions taken by the University to resolve the problem. He explained that the University formed a committee to investigate the charges made by Dr. Dong. At that time the committee had not completed its review but Mr. Schwartz assured Mr. Schriver that Stanford University would promptly notify NIH of any evidence of false research reporting uncovered by the committee. In subsequent telephone conversations with Stanford officials, NIH was informed that the committee had concluded that Dr. Lucas was not guilty of fallacious research reporting but perhaps was derelict in not maintaining good laboratory administration.
>
> Our actions in dealing with Stanford University in these matters are prompted by the fact that grants and contracts are awarded to the University which is responsible for the propriety and accuracy of the research and the subsequent reporting. In addition, because of the volume of awards made each year it is impossible for NIH to screen and validate all the information sent to us. As a consequence we must rely on the integrity of the University to apprise us of wrongdoing on the part of its research staff.

If pressed further, the NIH refers the matter to the Division of Management and Survey Review (DMSR) to conduct an investigation. This unit consists of accountants and other laypersons with no apparent expertise in scientific research or the evaluation of scientific data. These individuals have been assigned the task of gathering documents and interviewing witnesses (Currens 1977; Raub 1981). Hence, in contrast to the technical evaluation of proposals, where it is mandated policy to use groups of non-government consultants with expertise documented in the field under consideration, the policy here is to have administrators perform the technically demanding task of evaluating discrepancies in technical detail among publication, raw data, and claims to NIH. Not only does the NIH fail to use their panels of experts, but the policy has been adopted that such information will be withheld from them during the technical evaluation of any proposal that might be before the study section. This practice certainly raises questions about the quality of the evaluating process since the NIH administration indirectly assists the perpetrator of frauds by withholding information upon which a study section might reject the proposal.

Defects in NIH Policy

The NIH has recognized that it has a duty to protect the public's interest in the distribution of funds for scientific research (Fredrickson 1981). But, numerous exposés of scientific fraud during the six years after this author first called the NIH's attention to the problem indicate that NIH policies have failed that duty. In my opinion this state of affairs flows directly from NIH's reluctance to recognize the criminal nature of submissions of false data and statements to NIH's agents in the course of applications for financial assistance. The applicable law will be discussed in the next section. The analysis in this section, which is based upon the available public records, deals with more specific defects whose correction, I believe, will result in more satisfactory fraud detection and deterrence.

First, the reliance of a government agency on the "integrity" of client institutions to inform it of any wrongdoing is both misplaced and improper. It represents a position time-warped at least 20 years past for it fails to take into account the institution's strong economic self-interest in minimizing exposure.

From public records of the investigation of the four previously indicated institutions, only Boston University initiated contact with NIH to divulge evidence of data falsification to a NIH supported organization. And that was only after the researcher had resigned from the University Hospital staff. In the Yale case, while it is true that the study section was informed on May 20, 1980 that an audit of raw data revealed discrepancies requiring the retraction of publication, there was no indication of the nature of the cause of the discrepancies, to wit, that the data were plagiarized and falsified (Felig 1980). The NIH-supported investigator, one Dr. Soman, telephoned the NIH to indicate a change in "career" to administration. The Chairman of the Department of Medicine called to indicate only that the investigator was leaving Yale and to inquire what the options were for continuing Soman's current grant. The ensuing correspondence between NIH's Ms. Jean Curran and Yale extended through August 14, 1980, when, under a typewritten memorandum granting approval of a six months extension, appeared a poignant handwritten note indicating that they (NIH) had finally learned from an article in The New York Times (Altman 1980) of the nature of Soman's misrepresentations. The extension was put on hold and later disapproved.

In the case of Massachusetts General Hospital, Dr. Lamont-Havers, the Director of Research, requested that the NIH issue a "phase out" grant in February 1980 to last until August 1980, indicating to the NIH only that Dr. J. C. Long was departing for "personal reasons." The nature of his transgressions was withheld from NIH until after the continuation had been granted and reports appeared in the media. This is the same Dr. Lamont-Havers who had closed the Stanford investigation to avoid interference with internal matters of the University.

As for Stanford University, it never did disclose to the reviewing peer groups or any other NIH personnel, while still forwarding grant applications on behalf of Dr. Lucas (and eventually receiving funds greater than $1,400,000 [FBI memo 1977]), that the 1975 faculty committee had found:

> (his) work contained numerous errors, contradictions, and alterations and therefore that it did not meet the standards considered appropriate to scientific work or to the

> education of students under his purview.

or that

> As much of the impropriety resulted from scientific and administrative incompetence as from deliberate attempts to fabricate. (Stanford University, Feigen Committee 1975:2).

Contradicting what Stanford told the NIH, which caused NIH to close its file, the recommendation of the faculty committee was that there were sufficient grounds for a formal hearing to adjudicate what the committee mistakenly believed were the required mental elements of legally defined fraud (e.g., "intent" or "motive"). But even without proving scientific fraud, it is clear that withholding information from the NIH to the effect that a person was scientifically incompetent and that his "scientific" work contained alterations, numerous errors and contradictions, while still continuing to forward funding applications, is to corrupt the peer review system through concealment of material facts.

The acts recounted above are acts of the Institutions and not of the individual investigators. Thus, the position taken by some that there exists a relationship of "trust" between institutions and the NIH (see McCarthy 1982 and printed herein) is basically wrong. A trust relationship exists when, at the minimum, both parties adhere to the same high plane of conduct. The conduct of the previously cited Institutions is more consistent with the standards of the corporate marketplace: Caveat emptor, or let the buyer beware. Institutions have typically stonewalled the NIH unless faced with legal or other threats of compulsion.

The act of delegating decision-making authority to university-appointed committees is an equally serious defect. It is improper in light of the rules which govern the selection and conduct of study section members. These latter rules exclude those individuals from the evaluation process with any type of economic, blood, or organizational ties to the subject matter. Furthermore, study section members must be recognized experts in the field of study in question. University appointed committees certainly do not meet the same criteria of expertise, objectivity, and independence. Institutionally-appointed committees, perhaps uniformly, fail the test of independence and appear to have a conflict of interest in that they implicitly act on behalf of their employer. The makeup of the committees tends to fulfill the perceived needs of the institution rather than to meet NIH requirements, and, therefore, the scientific expertise with respect to the subject matter is certainly suspect.

The last factor to be discussed concerns the acquisition of evidentiary proof and the technical expertise required for its evaluation. Supervisors, perpetrators, and even legislators recognize that science fraud, which is the misrepresentation of purported observations, is both detected and proved by comparing the claims made against the raw data. Further, in view of the very serious nature of the transgressions and the length of time that the perpetrators have lived with their frauds, it must be recognized that attempts to alter the records may take place. Therefore, the proper procedure is, firstly, to impound notebooks and other physical records before there is an opportunity to further falsify the record, and secondly, to have different types of expert evaluations of the documents. These would include: (1) practicing scientists familiar with the specific subject matter; and (2) forensic scientists skilled in the examination of altered documents.

During the questioning at the Fraud Hearings, Congressman Walker elicited the admission from John Long, M.D., that had he been asked for the raw data he would have been exposed by the peer review process. Dr. Philip Felig (1981) also testified that in February 1980 an audit of original research data performed by an outside research scientist revealed marked discrepancies between the raw data and the matter published by Dr. Soman and himself. When confronted with the findings of the audit, Dr. Soman admitted to "doctoring the data." In addition, a second outside audit of subsequently-impounded records revealed that no original data could be found for nine of 14 papers which had been published.

Mere access to the laboratory notebooks and confrontation is not always sufficient, however, in determining the truth when dealing with people living with their frauds for several years. Some skill on the part of the interrogator is required. Long's undoing is instructive:

> Quay computed the expected rotor count. In each case, the count in the logbook was a fraction of what it should have been. An innocent explanation might have been that the machine had shut down through overheating before the run was completed. So Quay first asked Long if the machine was still running when he came back to it. Long said, yes, it was. Quay then explained to him and McCluskey why the inconsistency of the data in the logbook meant that the experiment could not have been undertaken as described... He admitted that this was a mistake made under great pressure... (Wade 1981:1022).

Next, there is the question of the alteration of documents. Dr. Felig was asked if there were any means by which editors and senior investigators could guarantee a publication. Felig responded by first asking rhetorically if one should be required to submit original data books, but then warned that even if there were such submission practices there might be those individuals who would falsify such sources of data, and then how could one be sure?

The answer to this is that two other skills must be available. One is the skill of a scientist with experience in the particular field, as illustrated again by Long's undoing:

> The light was hitting the pages of the notebook at an angle which showed me something I had never seen before. Taped onto the page were photos of Ouchterlony plates. The light was throwing into relief a ridge underneath the tape. I removed it and saw there was part of another

> piece of tape underneath, as if the photo had been torn out of some notebook and taped into this one... What Quay then discovered was reason to suppose that the pertinent section of the notebook had been forged. (Wade 1981:1022)

The other skill is that of forensic scientists, as described below (Klinger 1981):

> Parker [a forensic scientist] was hired by A. Baronio Martins to examine six laboratory notebooks that Martins obtained from Lucas' laboratory through court order...
>
> Parker studied the notebooks with a microscope and a number of sophisticated instruments,...
>
> In a deposition, he said someone had gone through the notebooks and added records of fictitious rats to the real ones....at least 32 percent of the listed rats never existed. In a number of instances, he said, the survival records of real rats had been altered, making them appear to have longer or shorter lifespans...
>
> Pointing to a listed rat whose life allegedly was lengthened, he said, "A cross means the animal died, but yet as we go on and look at Page 39A of the survival record, he goes on to live for 52 more days and then he dies again."
>
> Parker said non-existent rats and rats that died twice appear 'to a high degree' in reports Lucas filed with the NIH, in a letter Lucas sent to one of the Stanford committees that investigated his work, and in a paper by Lucas and Enomoto that was published in the journal 'Transplantation.'
>
> The Transplantation article has a table listing five rats in one experiment. 'My studies show that none of those five animals existed,' Parker said...
>
> In effect, it becomes science fiction," he said. (Klinger 1981:7a, col. 2)

The NIH obviously plays a pivotal role in the process of detection and prosecution because it has access to the scientific expertise required to evaluate the truthfulness of claims made to the NIH in applications for funding. And yet there is no case if there is no evidence, and there is no evidence if the raw data necessary to support or contradict the claims are not acquired by the investigating agency. Thus, ultimately, the failure of the NIH to collect and examine raw data is to preclude any possible prosecution, even in the types of cases recounted above. Those cases show that the perpetrators did not merely falsify the information in the publication or grant applications, they also left trails of botched attempts to alter and/or destroy the evidence of wrongdoing in the original records. This failure to obtain raw data has literally cost the American public the productive use of millions of dollars of scarce research funds, and, in addition, it has permitted the introduction of fictitious data into the scientific literature.

The Statutory Basis for Prosecution of "White Coat" Crime

There is general consensus that falsification of data is: 1) unethical; (2) reprehensible; (3) injures individuals, institutions, and the profession; and (4) wastes public funds. The real question, however, concerns by what legislative authority can action be taken, and what are the specific elements of the statute which must be proved to obtain a conviction?

The general prohibition against lying to the United States Government is provided for in Title 18 United States Code Section 1001. I will here review the court interpretations in the context of obtaining money from the Federal Government for support of academic research.

> **Title 18 United States Code Section 1001 provides:**
> Whoever, in any matter within the jurisdiction of any department or agency of the United States knowingly and willfully falsifies, conceals or covers up by any trick, scheme, or device a material fact, or makes any false, fictitious or fraudulent statements or representations, or makes or uses any false writing or document knowing the same to contain any false, fictitious or fraudulent statement or entry, shall be fined not more than $10,000 or imprisoned not more than five years, or both.

The invocation of criminal justice sanctions is an extraordinarily serious matter. While prosecution of instances of science fraud will have salutary effects on research and the effectiveness of public expenditures in general, judgments as to the quality of research and the determination of the importance or triviality of any particular piece of work are best left to evaluations by peers and tested by the course of the passage of time. In my opinion, the invocation of criminal sanctions is justified where, as discussed here, the dishonest acts are intended to and do circumvent the public policies established in our peer review system, and thereby corrupt the integrity of the decision-making process for personal or organizational gain.

The discussion of this section of the law should be of interest to all NIH applicants for since 1979, with little fanfare or explanation, the following section has been explicitly cited in DHHS Grant Application form PHS-398 Rev. 10/79 block 18:

> 18. CERTIFICATION AND ACCEPTANCE: I certify that the statements herein are true and complete to the best of my knowledge, and accept the obligation to comply with Public Health Service terms and conditions if a grant is awarded as the result of this application. A willfully false certification is a criminal offense. (U.S. Code, Title 18, Section 1001.)

As will be made clear from my discussion of judicial construction of section 1001, the proscription is far greater than only false certification.

JUDICIAL CONSTRUCTION

The Statement Element

The first and second elements to be proved are that a statement was made and that the statement was false. The statement portion of section 1001 is divided into three clauses:

> 1) falsifies, conceals or covers up by any *trick*, *scheme, or device* a material fact,
>
> 2) or makes any false, fictitious or fraudulent *statements* or *representations*,
>
> 3) or makes or uses any false *writing* or *document* knowing the same to contain any false, fictitious or fraudulent *statement* or *entry*,
>
> [emphasis supplied]

The definition of those acts which is encompassed by the statement section is quite broad and covers virtually every type of communication with the United States, including oral and written statements, data, entries and signatures. The statement need not be a sworn statement. This feature distinguishes the false statement proscription from perjury, a false sworn statement with which the public is more familiar. Statements for which there can be convictions under section 1001 are not limited to those required to be made by law or regulation. They may have been made in response to a government request.[1] [2] [3]

The breadth of the meaning of "false representations" is illustrated by a case in which the defendant, an accountant, signed Treasury checks with his clients' names without their knowledge and with "by R. Milo Gilbert, trustee." The defendant contended that he was their trustee, that the signature was his, and therefore there was no falsehood involved. The Court held that what was false was the assumed representation that Gilbert had the authority to do so.[4]

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[1] Marzani v. U.S., 83 U.S.App.D.C. 78, 168 F.2d 133 (1948) affirmed by equally divided court, 335 U.S. 895, 69 S.Ct. 299, 93 L.Ed. 431 (1948)

[2] Neely v. United States, 300 F.2d 67 (9th Cir. 1962) U.S. cert. denied 369 vs 864, 82 S. Ct. 1030 (1962)

[3] Cohen v. United States, 201 F. 2d 386 (9th Cir. 1953)

[4] Gilbert v. United States, 359 F. 2d 285 (9th Cir. 1966); U.S. cert. denied 385 U.S. 882

It appears clear, therefore, that the section 1001 encompasses both the statements made to and the data submitted to the NIH in the course of a scientist's application for funds or in response to the requirements for progress and final summary reports following successful application. The section would also appear to be applicable to the falsification of one's bibliography, since as NIH officials (Fredrickson 1981) have testified, the first critical step in evaluating a proposed research project is peer review, and this rigorous assessment includes an evaluation not only of the research problem itself, but also the credentials of the applicant investigator--including publication record, which figures heavily in this review.

Assuming, arguendo, that the investigator-applicant used falsified data in a publication (Long 1981) and subsequently cited that publication as a measure of their productivity in an application or progress report, the holding in the case mentioned above, Gilbert, compels the conclusion that a false representation was made. As in Gilbert, it is true that there was a publication, but also as in Gilbert, the false representation was that the data in the publication was actually acquired.

The Knowing and Willful Element

The third element of the offense requires proof of a subjective state of mind, which brings us to a brief discussion of the two basic parts of a criminal offense, the actus reus and the mens rea. These terms derive from the maxim, "Actus non facit reum, nisi mens sit rea," which translates to: an injurious act done without an evil state of mind is not a crime.

The actus reus is the physical act that is prohibited, here the making of a false statement. There must be some act, for fundamental to our jurisprudence is the notion that having evil thoughts alone is not punishable.

The mens rea is the mental state associated with the physical act. Mens rea has two meanings: (1) the general notion of blameworthiness in committing some prohibited act; and (2) a specific state of mind in committing a specific prohibited act. The specific mental state is crucial to defining the act to be punished. For example, receiving stolen goods is punishable only if it is proved that the receiver knew the goods were stolen.

The false statements provision in the criminal code prior to 1934 had words of purpose and intent, "for the purpose and with the intent of cheating and swindling or defrauding the Government of the United States." The words "intent to defraud" were specifically omitted from the last substantive revision of section 1001.[5] Therefore, to prove this element it is not necessary that the Government prove that the defendant in fact had an evil intent. The word "willful" means no more than that the forbidden act is done deliberately and with knowledge. The knowledge required to be proved is knowledge of falsity rather than knowledge of unlawfulness.[6] [7] [8] While the term "fraud" has been used in discussions of data falsifications, the mental element required in a false statements offense is knowledge and willfulness, not proof of an intent to deprive another of their property or right. The two occurrences of the word "fraudulent" in the statute appear in the disjunctive, "false, fictitious or fraudulent." Thus, proof of an intent to deceive is sufficient but not necessary for conviction under this statute.

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[5] United States v. Gilliland, 312 U.S. 86, 93, 85 L. Ed. 603 (1941)

The Jurisdiction Element

The fourth element required to be proved is that the false statement concerns a "matter within the jurisdiction of the United States." The Courts have generally construed the jurisdiction requirement very broadly. Based on the following review, the submission of false scientific data by an investigator funded by NIH would be a "matter within the jurisdiction of any department or agency of the United States," even when the submissions are to private professional publishers. The Supreme Court has held that a "statutory basis for an agency's request for information provides jurisdiction enough to punish fraudulent statements...because there is a valid legislative interest in protecting the integrity of official inquiries."[9]

The statutory basis for a request for information from applicants for federal funds is Section 301(c) of the Public Health Service Act, 42 U.S.C. section 241(c), and authorizes the Secretary of HEW to:

> Make grants-in-aid to universities, hospitals, laboratories, and other public or private institutions, and to individuals for... research projects.

The award of research grants under the above provision is governed by 42 C.F.R. part 52. With respect to evaluation and disposition of research grant applications, 42 C.F.R. section 52.13(a) states:

> (a) Evaluation. All applications filed in accordance with section 51.12 shall be evaluated by the Secretary... The Secretary's evaluation shall take into account among

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[6] Corcoran v. United States 229 F. 2d 295 (5th Cir. 1956)

[7] Neely, supra

[8] McBride v. United States 225 F. 2d 249 (5th Cir. 1955)

[9] Bryson v. United States, 396 U.S. 64, 24L. Ed. 2d 264, 90 S. Ct. 355

> other pertinent factors the scientific merit and significance of the project, the competency of the proposed staff in relation to the type of research involved, the feasibility of the project, the likelihood of its producing meaningful results, the proposed project and the amount of grant funds necessary for completion..."

In order for the Secretary to make such determinations, he/she must have the power to request information and that power is jurisdiction enough to punish the receipt of false information under Section 1001.

The false statement need not be made directly to an agency. The statement need only concern a matter over which the agency has power to act. Thus, for instance, a defendant may be convicted if he/she has submitted a false non-communist affiliation denial to the security officer of a private employer doing business with the Air Force.[10] Dependents have used the argument, in submitting bills to intermediaries such as Blue Cross for services to patients who are covered under the Medicare program, that the acts were directed towards private parties rather than towards the Government, and, therefore, the acts did not meet the "within the jurisdiction" requirement. Here, the fiscal intermediary, stands between the defendant and the Government. Nonetheless, section 1001 has been held to apply for submitting false statements in connection with the provision of services.[11] [12]

Where the proscribed statements are made to a private entity, the demonstration that the defendant contemplated that the statement was to be utilized in a manner which was within the jurisdiction of such department or agency is sufficient to be prosecuted.[13] The same reasoning was applied in Gilbert, supra, where the Treasury checks were endorsed and submitted to a bank to be credited to the defendant's personal account. The defendant asserted that he submitted the checks to a private entity and not the Government. The Court held that the matter was within the jurisdiction since the defendant certainly was aware that the endorsement of the checks was the first crucial step in their journey to the Treasury Department.

In two immigration cases, letters containing false offers or guarantees of employment from U.S. employers were sold to aliens attempting to obtain entry to the U.S. In both instances the jurisdiction requirement was met by finding that the defendants (i.e., the letter sellers) knew the letters were to be presented by the aliens to

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[10] Ogden v. United States 303 F. 3d 724 (9th Cir. 1962)

[11] United States v. Carey, 475 F. 2d 1019 (9th Cir., 1973)

[12] United States v. Matanky, 482 F. 2d 1319 (9th Cir. 1973) U.S. cert. denied. 414 U.S. 1039; U.S. reh. denied 414 U.S. 1138

[13] United States v. Candella, 487 F. 2d 1223 (3rd Cir. 1973) U.S. cert. denied 415 U.S. 977

Consulate and Immigration officials to secure visas.[14] [15]

The standard of a "matter within the jurisdiction" is whether or not the act concerns a matter over which the agency has power to act. Congress has provided that the merits of research and the competency of scientists are to be judged for the most part by the scientific community. In so doing, Congress delegated a very significant portion of the authorized function of government to nongovernmentally employed scientists. A key factor in the evaluation of an applicant is the applicant's productivity, and NIH officials admit such productivity is measured by the applicant's publication lists. The NIH relies on the evaluations of the private professional publisher to pass judgment on the merit of past scientific work in the course of determining the competency of an investigator to receive NIH funding. Clearly, it is within the contemplation of the investigator that publication is the "first step" on the route to such funding awards. Scientific data are also used by other scientists funded by NIH. The publication of false data would mislead others who are pursuing an "authorized" function of a governmental department when doing research under such government funding. Thus, Title 18 U.S.C. section 1001 appears to be peculiarly applicable to acts of publication of false scientific data when the data are submitted to private publishers and subsequently cited in applications of reports or directly in the applications themselves.

The Element of Materiality

The last element to prove is the element of materiality. The test for materiality appears to have been adopted by several Circuits from a case in which the defendant argued that because his application to the Veterans Administration had been rejected, the false statements contained therein were not "material" because the exercise of a government function could not have been influenced. The Court said that the conduct Congress intended to prevent by section 1001 was the willful submission to federal agencies of false statements calculated to induce agency reliance or action, irrespective of whether actual favorable agency action was, for other reasons, impossible. Therefore, the test is the intrinsic capabilities of the false statement itself, rather than the possibility of the actual attainment of its end as measured by collateral circumstances."[16] [17]

Hence, by statute, the misrepresentation must be of a material fact, and materiality is not necessarily measured in terms of pecuniary loss. The legislative history of the current language indicates that the President, in 1934, specifically rejected language which limited the application of materiality to pecuniary loss. Subsequent to the veto, the current language was enacted and adopted. The Supreme Court, recognizing the legislative history, read into the statute a congressional intent to protect the authorized functions of governmental departments and agencies from the perversion which might result from deceptive practices. The statute was used to control false reporting of oil supplies, or for the prosecution of the "presentation of false reports."[18]

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[14] United States v. Valdez, 594 F. 2d 725 (9th Cir. 1979)

[15] United States v. Williams, 616 F 2d 759 (1980) U.S. cert. denied 449 U.S. 857

[16] United States v. Quirk, D.C.E.D.Pa 167 F. Supp. 462, 464 (1958), (affirmed 3rd Cir. 266 F. 2d 26; cited with approval Brandow, infra, at 565)

[17] Brandow v. United States, 268 F. 2d 559 (9th Cir. 1959)

The following cases are examples of prosecutions under this statute in which there was no pecuniary loss to the Government. Nevertheless, the test for materiality was met in each instance. In one case, the investigators of the Drug Enforcement Administration asked the defendant whether he had made any out-of-state purchases of a controlled substance, when the investigators already had such evidence. The defendant argued that since the investigators could not be mislead by a false denial, the statement was not materially misleading. The Court held that the key question was the intrinsic capability of the statement and not the possibility of actual attainment of its end.[19]

In another case, the defendant argued that a denial that he had been overseas did not constitute a material falsehood since when the Customs agent searched him and found contraband, the contraband was not allowed into the United States. The declarant was claiming the privilege of entry, the Court held, and his statement potentially impaired the function of the Customs Service.[20]

In a third case, the defendant argued that the alleged false statements on an application for an agricultural cooperative association exemption in an Interstate Commerce Commission regulatory scheme was not material because the ICC regularly conducts an investigation to verify the statements, and, therefore, the ICC did not rely on the specific statements. After stating that proof of actual reliance was not needed, the Court pointed out that as a result of filing the application, the carrier was permitted a year of operation before the ICC got around to completing the investigation and taking steps to enjoin further activity by the carrier. Therefore, the Government used actual induction of reliance as proof of meeting the test of "intrinsic capability" of the false statement to induce reliance.[21]

It has been observed: "the potential volume of litigation in section 1001 cases is virtually unlimited. At the same time, the significance of the typical case, by almost any measure of importance, is virtually nil. Consequently, these cases are routinely declined by the Justice Department. Only under 'aggravated circumstances'--generally defined in terms of dollar loss to the government--is prosecution undertaken." (Prosecutorial Discretion 1972)

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[18] Gilliland, *supra*

[19] United States v. Goldfine, 538 F.2d 815 (9th Cir. 1976)

[20] United States v. Rose, 570 F.2d 1358 (9th Cir. 1978)

[21] United States v. Talkington, 589 F.2d 415 (9th Cir. 1978)

Publication of false scientific data is not de minimus. Publication is the end result desired of the Federal program. Indeed, the government requires at the termination of a particular project that the investigators report their plans for future publication. Nor can it be argued with any force that there is a lack of aggravated circumstances for even a dollar standard, which is not required under section 1001, is fully met. Courts have found no difficulty in holding that a false voucher of fifty-six dollars was sufficient for materiality,[22] and, in the types of cases discussed here, it has been demonstrated that millions of dollars have been awarded to institutions and tens of thousands of dollars in salary and other benefits have been awarded to individuals who have submitted false statements and published fictitious data.

In summary, the submission of false claims of data acquisition, withholding of data adverse to a theory or hypothesis, and false citations of publications are material within the meaning of section 1001 since the acts are all calculated to induce reliance, although in fact they may not, by scientists and federal science administrators with respect to the competence of the investigator in pursuing an authorized function of government: the acquisition and dissemination of scientific knowledge.

Title 18 U.S.C. section 1001 "was intended to serve the vital public purpose of protecting governmental functions from frustration and distortion through deceptive practice."[23] Through the provisions of the Public Health Act, Congress has implicitly adopted the "Truthful Full Disclosure Standard" of Science as the standard for the conduct of scientists operating under federal biomedical research grants and contracts. Knowing publication of false data in research publications is a violation of that standard.

The Supreme Court has said:

> it cannot be thought that as a general principle of our law a citizen has a privilege to answer fraudulently a question that the Government should not have asked. Our legal system provides methods for challenging the Government's right to ask questions (fn omitted)-lying is not one of them. A citizen may decline to answer the question, or answer it honestly, but he cannot with impunity knowingly and willfully answer with a falsehood.[24]

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[22] United States v. East, 416 F. 2d 351 (9th Cir. 1969)

[23] Gilliland, supra

[24] Bryson, supra

This reasoning compels the proposition that if one cannot make false statements even in response to improper questions, then one cannot make false statements to the U.S. *ab initio* to obtain a benefit from the U.S. Thus, Title 18 U.S.C. section 1001 is applicable to remedy the injury to both the tangible pecuniary and non-pecuniary scientific interests of the public flowing from scientific misrepresentations made in the course of research programs supported by the NIH.

ACKNOWLEDGMENTS

This work was submitted initially in two parts in fulfillment of course requirements, first to Professor Aidan Gough in December 1980 and subsequently to Professor Dorothy Glancy in May 1981, while the author was matriculated at the University of Santa Clara School of Law, Santa Clara, California. Their assistance and direction are gratefully acknowledged.

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PART FOUR

The Depletion of a National Resource

In Chapter 9, the Kilbournes treat science as a vital national resource and deviance in science as the depletion of that resource. Science is specifically examined where it interfaces with other institutions in our society and with other societies. They argue that the costs of scientific deviance are greatest along the "external frontiers" of science, thus confirming the central role of science in contemporary society.

DEVIANCE IN SCIENCE: THE DEPLETION OF A VITAL NATIONAL RESOURCE

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In a recent address to the Annual Meeting of the American Association for the Advancement of Science (AAAS), outgoing president D. Allan Bromley (1982) discussed the interface of science with other institutions in our society and with other societies. This interface was referred to as the external frontier of science and was described in terms of those relations or linkages with the federal government, the educational establishment, the private sector, national defense and security, world science/technology, the developing world, and U.S. society itself. Bromley (1982) argued that these frontiers are no less important than the internal frontiers of science or "those frontiers where human knowledge is pushed toward the unknown" (Bromley 1982: 1035). From this perspective, science and technology are considered to play a vital role in nearly every aspect of our lives, whether it be in relation to health, education, commerce, defense, trade, etc. Indeed, most of us would agree, science has assumed an increasing role in all of our lives to the point that science and the science industry must be regarded as a vital national resource which is necessary for our survival.

Although science is clearly a vital national resource essential to our survival, there is a further implication of the Bromley thesis in need of elaboration. That is, *given the centrality of science in our lives, it is of the utmost importance to recognize that any activity or relation which minimizes or diverts support for science is in effect depleting or potentially depleting, and conceivably exploiting, a vital national resource*. Bromley has suggested science is endangered during periods of scarce resources, rising costs and increased competition from other national priorities. However, there are other threats to science, some of which may be most visible during periods of retrenchment and rapidly diminishing resources.

One such threat or problem confronting scientists in particular and society in general is deviance in science. Deviance in science has recently been discussed in terms of innovation, occupational deviation and white-collar crime (Kilbourne, Kilbourne & Higgins 1981, and printed herein). While it is true that methodological and theoretical innovations can be thought of as forms of deviance in science from a broad perspective, it is really the consideration of occupational deviation and especially white collar crime which poses a threat. Occupational deviation consists of deviance from those norms specific to science and includes *ad hominem* attacks, dogmatism, shoddy work, secrecy, milking data, unnecessary delays in publication and publicity seeking. White-collar crime consists of those categories of societal crimes committed in the course of one's profession for which there are explicit penal sanctions (e.g., fraud, misappropriation of funds, discrimination, plagiarism, and the violation of protection of human subjects, etc.).

When we take the preceding into consideration, certain forms of scientific deviance are seen to present a threat to both the scientific community and to the larger society. Occupational deviance and white-collar crime in science may result in the depletion and/or exploitation of a vital national resource (i.e., science). Most importantly, the costs of deviance in science may be greatest where science interfaces with other institutions and/or societies.

Thus, the purpose of the present paper is to examine how deviance in science decreases the likelihood of achieving need-related goals along each of the external frontiers of science. The costs of deviance in science are investigated in relation to the interface of science with: (1) the federal government; (2) the private sector or industry; (3) the educational establishment; (4) national defense and security; (5) world science and technology; (6) the developing world; and 7) U.S. society.

The Interface with the Federal Government

The basic need of science along the external frontier with the Federal government concerns the issue of continued fiscal support of basic research and development. Since World War II the Federal government has essentially assumed the responsibility for financing research and development due to: (1) the special and oftentimes narrow interests of the private sector; (2) the risks associated with long range research; (3) the scale and magnitude of instrumentation required; and (4) the "unbusiness-like" nature of basic research (i.e., the tendency to deemphasize short-term evaluation and payoffs [Bromley 1982]). However, the role of the federal government in financing research must be appreciated in relation to the general increase in competition for federal support. Research and development must compete with highway construction, defense, social security and other entitlement programs, etc., and the fight is getting tougher all the time (Norman 1982; Walsh 1980).

It is precisely at this juncture of increased competition with other programs for limited Federal funding which marks the critical or stress point of the interface of science with the Federal government. And it is at this point where deviance in science threatens the dollar flow to science. The increased demand to reduce the budget, continued pressures from other special interest groups, and increased overall demands for federal support may compel the Federal government to reduce total expenditures on R & D and/or selectively, and sometimes arbitrarily, fund only certain kinds of research when reports of deviance in science come to light.

Public reports of deviance in science (Begley, Malamud & Hager 1982b; Golden 1981; Hunt 1981; Broad 1980a, 1980b, 1981b, 1982a), congressional investigations of the same (Broad 1981a; Marshall 1981;

Shapley 1977) and the felt need by NIH to develop a policy on "deviance in science" (Sun 1982b) have caused some congressmen to take a second look at R & D funding. Congressman Albert Gore, chairman of the Subcommittee on Investigations and Oversight of the House Committee on Science and Technology, has said, "In a time where federal research money becomes increasingly precious, common sense dictates that the federal government not squander it on individuals who have established their unworthiness and undependability" (quote in McDonald 1981:16). Deviance in science appears to make science-related budget cutting all the more easier.

Sensational reports of deviance in science have also led to a "credibility gap" for science and scientists (i.e., what science claims and what science actually does). Such public reports have tended to shatter the mystique surrounding science and to make scientists more human (Begley 1982) and therefore more culpable. Even if the actual proportion of reported instances of deviance in science are relatively small compared to all science-related activities, taxpayers and lawmakers may not perceive it that way.

In sum, deviance in science is a problem. Just how severe of a problem cannot be fully assessed at this time. However, one thing is certain, continued reports of scientific deviance and/or scientific cover-ups will make it increasingly difficult for scientists and the science industry to compete for federal funds; both in relation to other national priorities and among themselves.

The Interface with the Private Sector (Industry)

Some believe the central need of science where it interfaces with industry concerns the rebuilding of bridges of interdependence and mutual support. Industry must attract bright people and develop new technologies to remain competitive in the national and international markets. Academia must attract financial support and real-world input from industry to maintain its research and teaching activities as well as to secure opportunities for its graduates. A partnership between universities, industry and government is therefore necessary to insure economic growth and an improved international posture. This is all the more important since the proportion of scientists and engineers in the U. S. labor force has continued to decline since 1965 (see Bromley 1982).

Deviance in science poses the greatest threat along the external frontier with industry where it tends to interfere with or disrupt the rebuilding of bridges of interdependence and mutual support. Such scientific deviance is evident in the following: 1) the growing commercialization of scientific research and the resulting evolution of "trade secrets"; 2) the conflicts of interest of some university scientists involved in lucrative research; and 3) the tendency for some industrial interests to discourage research in areas incompatible with corporate goals and interests.

The growing commercialization of scientific research in some fields has made it evident (Begley, Lubenow, Abramson, Carey, Malamud

& Hager 1982a; Nelkin 1982), if there was ever any doubt, that the imperatives of the business community--profits, expanding markets, and the need to maintain a competitive edge--are often incompatible with the ideals of science (i.e., universalism, disinterestedness, organized skepticism, etc.). When science is steered by the profit motive, for example, it begins to resemble the business community and to develop rationalizations for maintaining "trade secrets" and the concealment of discoveries and breakthroughs which could be seized upon by one's competitors. The promotion of commercial secrecy may secure the competitive edge of a scientifically oriented research business, but it may simultaneously impede the advancement of science; knowledge becomes closely guarded and restricted. Begley et al. (1982a) have expressed this quite succinctly:

> Formerly, scientists shared their thoughts because they assumed that, having come up with an idea, they would be first to develop it. But with industry sniffing around for lucrative projects, says biologist Emanuel Epstein of UC Davis, "you can't mention something you're thinking about because a private firm can have five post-docs on the job the day after tomorrow." That fear has silenced some biology corridors. A UC San Francisco committee reported that because so many of its scientists were affiliated with Genentech, "people were loath to ask questions and give suggestions in seminars...there was a feeling that someone might take an idea and patent it." (p. 69)

Related to the growing commercialization of some scientific research are the possible "conflicts of interest" of some university researchers involved in lucrative forms of scientific research (Begley et al. 1982a; Shapley 1973; Wade 1976a, 1980). The area of genetic engineering, for instance, is especially relevant here and holds out long term consequences for both science and society. This is evident in relation to the recent development of recombinant DNA technology and in the production of hybridoma cells. Some genetic researchers may experience a conflict of interest between their university commitments to unfettered and basic scientific research and their concurrent investment in and obligation to private enterprise. Their business investments may sometimes require them to only selectively research and publish their new understandings and new discoveries (Nelkin 1982; Norman 1981). Begley et al. (1982a), for example, have argued:

> Commercialized biology imperils academia's role as impartial advisor on science policy. MIT's David Baltimore, for instance, is a Nobel Prize winner and one of biology's most respected members; he owns more than $2.5 million worth of Collaborative Research, Inc., a biotechnology firm, and serves on the committee that evaluates the safety of recombinant DNA for the government. With such ties, says Rep. Albert Gore of Tennessee, "Our ability to get advice on the policy implications of new discoveries is diminished." (p. 70)

Thirdly, scientists may be employed by industry to produce particular results for the purpose of maintaining the corporate status quo and to avoid, even suppress, the production of research that could jeopardize the same (Barnett 1980; Smith 1982a). Scientists may be employed to justify, using scientific and statistical methodologies and reports, the creation and maintenance of certain programs, products and/or activities which are self-serving and not in the best interests of the consumer or the scientific community (Kotelchuck 1975; Smith 1982a). In this capacity, scientists are literally "hired guns" to create the illusion of scientific credibility and respectability (for a more recent example see "Scandal in the Testing Lab" [Begley, Rotenberk, & Hager, 1983]).

An excellent example is evident in the controversy surrounding the use of the pesticide DBCP (i.e., this is a soil fumigant which has been used to control nematodes and microscopic worms that feed on crop roots). The DBCP controversy first came to light following reports of sterility and reduced fertility in workers who mixed and canned the pesticide at a chemical plan in Lanthrop, California (Whorton, Milby, Krauss & Stubbs 1979). A California state investigation (California Department of Industrial Relations 1977) of why the potent effects of this pesticide had been overlooked indicated a number of highly revealing facts. Numerous U.C. scientists were found to have ignored the hazards of DBCP and to have indicated their financial ties to Shell Chemical Corporation (Barnett 1980). Bert Lear, a U.C. Davis nematologist, never published that, "Studies in greenhouse and laboratory showed that tomato seedlings absorb DBCP through the roots and translocate it upwards" (quote in Barnett 1980:33) because that was not his purpose, nor did he and his graduate student attempt to assess how deeply down the chemical would leach (also see Johnson & Lear 1968, 1969). Yet, "after twenty years of use, DBCP has evidently leached down hundreds of feet to contaminate ground water supplies" (Barnett 1980:33).

The Interface with the Educational Establishment

Along the interface of science with the educational establishment the science community must concern itself with both increasing public scientific literacy and providing specialized scientific training (i.e., pre-professional and professional). Some believe one of the most pressing needs in the U. S. is the need to attract a larger proportion of the nation's youth into mathematics, science and engineering careers and to provide them with the appropriate educational foundations. The science industry must furthermore address this personnel shortage at a time when science has done a poor job in attracting women and minorities into mathematics, science and engineering (see Bromley 1982).

Notwithstanding, there are reasons other than the lack of an appropriate secondary education to explain the low level of scientific literacy in the U.S. Also, there are reasons other than poor recruitment strategies to explain why women still have higher unemployment rates and lower salaries than men in all fields of science, and why the proportion of minorities in science, mathematics and engineering is less today than ten years ago (Bromley 1982). There is some evidence which suggests that deviance in science has contributed, in part, to the low level of scientific literacy amongst some groups in the U.S. and to current personnel shortages in science.

Relating the issues of scientific literacy and personnel shortages to deviance in science begins with an appreciation of the inseparability of values, ideology and science (Sampson 1978). Although values will always play some role in science, it is when values become the chief steering mechanism guiding scientific research and explanation that the problem of scientific deviance most probably arises. The ability of the scientist to be objective and neutral is greatly diminished and the intent of the scientist to "let the dice fall where they may" becomes nearly impossible to achieve. In such cases, scientific findings may be manufactured to support particular values.

For instance, according to Dorfman's (1978) analysis of the Sir Cyril Burt scandal:

> Burt used his systematic constructions to test deductions from his genetic theory of social class and to answer criticisms of previous work. Since constructions are not data, it is extraordinary for Burt to have discussed his tables as if they were actual data. (p. 1184)

Moreover, Burt's research was subsequently used as one of the rationales for tracking working class children into the trades and hence denying them access to higher education. Thousands of Britain's working class families were affected, and Britain was denied a potential source of talent and future national wealth. This is a perfect example of how a single instance of deviance in science may have long range effects for both individuals and a nation.

Kamin has suggested that the uncritical acceptance of Burt's findings can be attributed, in part, to the underlying values inherent in them, as indicated by the statement: "Every professor knew that his child was brighter than the ditch-digger's child, so what was there to challenge" (quoted in Wade 1976b:918). Some scientists may thus conceivably manufacture data not only to confirm their theories, but also to confirm the values inherent in those theories.

The manufacturing of findings and/or contrived conclusions to support particular values are further suggested in some research on race differences. In a 1906 issue of the American Journal of Anatomy, (i.e., the aftermath of Reconstruction), Dr. Robert Bean argued in defense of racial segregation in education:

> Having demonstrated that the negro and the caucasian are widely different in characteristics, due to a deficiency of grey matter and connecting fibers in the negro brain, especially in the frontal lobes, a deficiency that is hereditary...we are forced to conclude that it is useless to try to elevate the negro by education or otherwise, except in the direction of his natural endowment. (quoted in Fishman & Wainer 1982:9)

Can we really presume such indefensible statements as the above did not function in the service of particular values and/or vested interests? Certainly when Goddard later darkened the eyes and mouths of his photographs of the Kallikak subjects he had in mind a value judgement about intellectual deficiency and immorality (Gould 1981). However, no rigorous genetic scientist would consider "doctored" data like that as legitimate support for the heritability factor.

Nor is the above any different from some scientific studies of sex differences. Some presumably scientific findings on sex differences have been so misused, for all extent and purposes, they might have been manufactured. Naomi Weisstein (1971) has expressed this most succinctly in relation to the selective use of primate studies to confirm certain sex differences:

> Invariably, only those primates have been cited that exhibit exactly the kind of behavior that the proponents of the biological basis of human female behavior wish were true for humans. Thus, baboons and rhesus monkeys are generally cited: males in these groups exhibit some of the most irritable and aggressive behavior found in primates, and if one wishes to argue that females are naturally passive and submissive, these groups provide vivid examples. (p. 219)

At issue are those scientific findings of sex or race differences which have relied on some form of error (Block & Dworkin 1976; Fishman & Wainer 1982; Gould 1981; Haraway 1978; Kamin 1974, 1976; Loehlin, Lindzey & Spuhler 1975; Pierce 1971) and have subsequently been used by others to justify existing relations in society (Bowles & Gintis 1973). What appears to be an over-reliance upon select samples, confounded variables, ambiguous or questionable data, overgeneralizations, problematic analyses and/or the tendency to overlook inconsistent findings, by those who should know better, has led some scientists to suspect the value-biased nature and therefore deviance of some scientific investigations. A suspicion which is heightened when the exaggerated claims of some are examined in relation to knowledgeable estimates of heritability (see Defries & Plomin 1978; Scarr & Weinberg 1977).[1]

Thus, when conceivably fraudulent scientific findings on class, race and/or sex differences are considered in conjunction with the widespread use, and oftentimes misuse (Lefrancois 1980), of I.Q. tests in the schools, we can see how such a situation may result in limiting the educational experiences of certain groups. Fraudulent scientific findings in conjunction with the misuse of I.Q. tests may function as self-fulfilling prophecies by convincing women, minorities and low income groups, etc., that they lack the ability to either understand or to enter professionally into the sciences (Beckwith & Durkin 1981). Instead of being seen as a small and dark side of science, fraudulent scientific findings may come to signify, for some, science itself (see Sampson 1982, and printed herein).

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[1] We should by no means limit ourselves to criticisms of hereditary positions. Take the case of Margaret Mead, for example, who was a cultural determinist. Derek Freeman has recently alleged that Mead intentionally misrepresented Samoan life in order to confirm her values and personal ideology (see J. Leo 1983:68-70).

The Interface with National Security and Defense

At the interface of science and national security some believe there is a need to develop new and enduring relationships between scientists and the military. This would necessitate putting the universities back into the defense business, encouraging the Department of Defense (DOD) to support long-term basic research and secondary education, and also creating a partnership between universities and the DOD toward the goal of meeting military personnel needs for research and operations. Some believe the need for new relationships between scientists, universities and the DOD is a top priority because the separation of the universities and the DOD has frequently hurt both. However, there are certain inherent tensions in such relationships in that national security often demands secrecy while science demands free and open communication (see Bromley 1982).

Unfortunately, the problem of rebuilding the relationship between the DOD and the universities that once existed during the 1950s entails more than overcoming the conflict between "openness" and "secrecy," or even accepting the change in values of the 60s and 70s. Deviance in science at the interface of science and national security has adversely affected the relationship between scientists, the universities and the DOD.

There are two issues involving scientific deviance in relation to defense and national security, both of which violate the ideals of science, foster distrust and impede new relationships between science and defense. One issue involves the employment of scientists in national security-related activities that may require both a high degree of secrecy and conceivably criminal activities. Such pursuits tend to engender distrust on the part of scientists concerning what they fear they might be asked to do for national security and defense. The second issue involves the misrepresenting and self-serving activities by some scientists in national security-related activities and causes the defense industry and others to question what military scientists may ask to be allowed to do. Some examples will be given to illustrate these two issues.

First, public reports of violations of safeguards for the protection of research subjects by scientists doing research in the name of national security and defense has had an adverse affect on the relationship between the DOD, science and the universities. Violations of the protections afforded to human research subjects tend to strain relations between scientists themselves as well as with those who desire such services. For instance, Senator Frank Church's Select Committee on Intelligence stated in relation to CIA drug experimentation and their employment of scientists in this regard:

> The research and development program, and particularly the covert testing program, resulted in massive abridgements of the rights of American citizens, sometimes with tragic consequences. The deaths of two Americans can be attributed to these programs; other participants in the testing program may still suffer from the residual effects. (Szulc 1977:153)

The two Americans Senator Church spoke of were biochemist Frank Olson and tennis professional Harold Blauer. Both men were unwitting guinea pigs in CIA scientific research on psychedelic drugs and both committed suicide. No less disconcerting are recent reports that the U. S. military covered-up criminal and inhuman medical experimentation by Japanese scientists during W.W.II upon captured prisoners of war, some of whom were Americans, in order to secure the results of this experimentation (Powell 1981).

Second, scientists have sometimes solicited or encouraged funding from the DOD to research and develop national security-related activities and projects of questionable scientific value. One such technological misadventure was the quest to develop a nuclear plane. This ultimately doomed project spanned three decades and cost U. S. taxpayers more than one billion dollars. John Tierney (1982) reports that during the initial meetings, which were held to determine if a nuclear powered plane was feasible, many scientists were skeptical and suspected it would be a waste of money. Nonetheless, some of these same scientists were willing to go ahead and take advantage of the Air Force's enthusiasm. Events of this kind may not be unrelated to the exasperation and concern by some over the exponentially escalating costs of defense in the U. S. or the recent claim that the U. S. and its Western allies can no longer afford the costs of defense (Zuckerman 1982).

Thus, another form of scientific deviance concerns the self-serving nature of some scientific research on weapons and support systems, which is not scientifically justifiable given current knowledge and available resources. Some research and development may bring millions of research dollars into the hands of military scientists without the hope of ever achieving stated objectives or the criteria of scientific warrantability (Tierney 1982; Tsipis 1972; Zuckerman 1982).

In sum, scientific deviance at the interface of science and national security may involve the violation of the protection of research subjects and/or the rights of American citizens for the sake of national security. It may also involve the misrepresenting and self-serving projects of some defense working scientists. Both deviant practices foster distrust where science interfaces with defense and national security.

The Interface with World Science and Technology

The greatest need confronting science at its interface with world science and technology concerns the need for the increased participation of the U. S. in the international and technological communities. This would seem mandatory since the U. S. has already relinquished its preeminence in a number of important scientific fields (e.g., particle and nuclear physics, computer science, mathematics, etc.) and in light of the increasing demands being placed upon U.S. resources and the rising costs of research which necessarily precludes superiority in all fields. Today, we are making a lesser commitment to international science and technology at a time when we are falling further and further behind from our previous role of preeminence and leadership (Bromley 1982).

Interestingly, the greatest threat along the frontier with World Science arises not from deviance in science, but from deviance against science. By deviance against science, we mean the systematic deployment of ideology, and whatever resources which can be directed in the service of such ideology, toward the goals of minimizing, divesting and/or suppressing scientific inquiry. Deviance against science constitutes an attempt to suppress free, rational and critical inquiry because of the threat such activity presents to particular vested interests in science and/or society. An attempt is henceforth made to restrict the growth, development and maintenance of the scientific community.

Current opposition, in the form of laissez-faire ideology, to the development of an international community of science and technology constitutes a case in point. When laissez-faire ideology or similar arguments are used to oppose international science this should be thought of as a form of deviance against science. Those advocating such an ideology often have a vested interest in maintaining the present form of the scientific community, and their concern for money, power or prestige, etc., overrides their concern for scientific advancement resulting from cooperative projects between nations. In other words, certain individuals who subscribe to a laissez-faire ideology may misrepresent themselves as supporting science when, in reality, they only support a particular form of the science community.

A recent article by William Broad (1982b), on the possible collaboration of U.S. and European physicists in the development of a subatomic particle accelerator, can serve as a good example of the vested interests at stake in such cooperative ventures. Some of the ways this one particular cooperative project between nations would upset the "apple cart" are indicated by: (1) the approximate 20 million U. S. dollars which would be invested in Europe instead of in the U. S., and the continued dollar drain on U. S. based research in high energy physics; (2) the increase in international coauthorship and the subsequent decrease in national authorship and prestige; and (3) the further development of international data banks and shared knowledge that challenges the scientific as well as economic and political dominance of certain countries.

Kerwin (1981) has previously discussed the vested interests underlying some of the opposition to international science and has suggested that international science will probably result in: 1) a significant transfer of technology to developing nations and increased competition abroad; 2) an increase in the proportion of coauthored scientific papers between nations and a decrease in single nation preeminence; 3) threats to the vested interests underlying the disproportionate spending on particular health problems; and 4) continued opposition by most nations unless long-term commercial interests are involved. Also, Fleishman (1982) has commented, in relation to the U.S. withdrawal from participation in the International Institute for Applied Systems Analysis (IIASA), that arguments against international projects may focus upon the negative and ignore the positive. Concerning the IIASA project, opposition to non-military expenditures and presumed espionage activity overshadowed the gains resulting from the public policy projects of IIASA (e.g., dam and reservoir control and planning of agriculture resources).

In sum, laissez-faire ideology can be used to deviate against science by opposing the development of an international science community based upon cooperation and interdependence. Although laissez-faire ideologues claim a "hands off" policy and a reluctance for joint planning, they have interfered in international policy-making to thwart the development of international science. Laissez-faire ideology is used to support science when it serves certain vested interests and it is used to attack science when those same or similar interests are not being served. But of course, "the best context for science is the global community, and not unrelated is the fact that the global community is also the best hope for the future of humanity" (Kerwin 1981:1072).

The Interface with the Developing World

The central need of science at the interface with the developing world pertains to the application of science and technology for the purposes of facilitating social and economic growth. This is all the more important given the widening gap between the most affluent and the least affluent, the premature transfer of technology to the Third World, the failure of large international ventures, and the continuing brain drain from Third World countries to the developed countries (see Bromley 1982).

While Bromley views this "application" problem as resulting from shortsightedness, mismanagement, and, in part, ethnocentrism, another issue is clearly involved. Some developed countries have *disguised* their use of science in the Third World. Although claiming humanitarian concerns, some developed countries have sometimes used science in their relations with Third World countries to maintain their own economies and national interests (Hulse 1982). This poses a problem for all of science when scientific expertise is used to exploit and to control, rather than to advance knowledge and to insure development since the ideals of science are no less true for applied as for basic research. *Such misrepresenting and fraudulent practices constitute a problem of deviance in applied science with far-reaching consequences for the entire world community*.

For example, with regard to the self-serving nature of some programs designed to provide primary health care to developing nations, Martin Kaplan (1976), Director of Research, Promotion and Development of the World Health Organization, has made this observation:

> Assistance from advanced nations has often adversely influenced health activities and the development of health services in the poor countries... These programs with their concern for visibility not infrequently include spectacular but ill-adapted showpieces, such as large hospitals and advanced technical apparatus, and training fellowships in other countries in highly specialized aspects of clinical medicine and surgery. Thus, outside aid programs can serve to dissipate scarce local resources and skilled manpower and to promote a brain drain. (p. 30)

Bandyopadhyay and Shiva (1981) have forthrightly attacked the problem of the deviant applications of science in Third World countries. In their discussion of the growing scientific establishment in India, these authors claim that Western science is neither more rational, efficient or socially neutral. Rather, "development programs which claim to be for the upliftment of the people, have systematically, through forceful introduction of alien technologies, led to further impoverishment of the rural poor" (Bandyopadhyay & Shiva 1981:24). They cite case studies on the development of scientific technologies in relation to fishing and agriculture to make their point that science has not helped 80% of the rural poor and has only benefited the scientists and the industrial interests they represent. While claiming that science and technology are the key to solving poverty and backwardness, modern technologies have threatened traditional farming and fishing communities in India by depriving them of their source of livelihood (i.e., fish and/or land).

Wilson (1981), in relation to the infant formula problem, similarly claims:

> infant formula is yet another of many commodities developed by science to increase the profits and power of the few who direct it. (p. 18-19)

Mass media campaigns by industrial interests, with the help of doctors and medical scientists, continue to promote infant formula, even though epidemiological studies consistently show that for most people in the world breast-fed infants have lower rates of mortality and morbidity than artificially-fed infants (Wray 1981). The problem of iatrogenesis becomes a problem of deviance in applied science. Similar reasoning has led Rack (1977) to conclude in relation to some U.S. medical programs in Columbia:

> Medical systems and technologies from the industrialized U.S. are not valuable to a population ravaged by tuberculosis, malnutrition, parasitism and simple infections for which the immediate cures are simple but the underlying causes social and structural. (p. 25)

Some U. S. sponsored medical science projects in Columbia, for instance, were misleading because the beneficiaries were not Columbians, as many were led to believe. The beneficiaries, if any, were the consumers in western industrialized countries.

In sum, the self-serving nature of some applied science and technology in Third World countries has resulted in fostering dependency among those people and in literally destroying limited resources and ecological systems. Such programs serve the interests of the scientists themselves and the nations they represent. Applied science and technology in the service of self-serving interests is hence deviant from both the ideals of science (i.e., objectivity and disinterestedness) and the legal statutes of most societies (i.e., it willfully misrepresents what it can accomplish or will accomplish and is thus fraudulent).

The Interface with U.S. Society

The greatest need of science at the interface with U. S. society involves the responsibility of scientists to respond to the public's interest in science and to foster public support for science and technology. This can be accomplished by educating the public to support science on its own merits and by the public's recognition of the vital role played by science in contemporary society. Thus, scientists must make a major commitment to increasing public scientific literacy and, at the same time, have enough foresight to encourage input from sources external to the scientific community (see Bromley 1982).

It is an interesting irony that a major threat to public support for science should come from science itself. This is most apparent in the stereotypic response of some scientists to public allegations of scientific deviance and in the increasing tendency to "politicize" and "commercialize" science in contemporary society. Such trends are deviant from the ideals of science.

The stereotypic response of some scientists to public allegations and reports of deviance is often circumspect since it tends to violate the presumption of objectivity, disinterestedness and neutrality of scientists. More disparaging yet, it resembles the all too typical response of those in our culture who get caught with their hands in the "cookie jar." This stereotypic response is characterized by: (1)disclaimers--the tendency for some scientists to deny on face value alone the occurrence, magnitude and/or alleged frequency of public reports of scientific deviance (see Broad 1980b, 1981a, 1981b); (2) excusatoy accounting--a form of account-giving whereby the person acknowledges the wrongfulness of their actions, but denies responsibility for it (Scott & Lyman 1968). Scientists may employ excuses to minimize their responsibility for deviance and may sometimes accompany their excuses with an attempt to search for scapegoats (see McKean 1981; Silberner 1982). Such excusatory accounts are indicated by the following:

> My error was not in knowingly promulgating false data, but rather in succumbing to extensive pressure placed on me by the Institute director to publicize information regarding rabbits... In fact, for a considerable period

> of time I have been under extreme personal and professional stress which led to both mental and physical exhaustion. (quote of Summerlin in Culliton 1974:1157)
>
> He [Haas] testified that he had forgotten to pay back the university for the trip because of the strains on his marriage and pressures on him to write a 300-page report following the Hong Kong trip. (Shapley 1978:418)
>
> Darsee said, "I did you dirt. I'm deeply sorry"....He was also, he said, under great stress: he had not taken a day of vacation in 6 years and was worried about his hospitalized mother. (Begley, et al. 1982b:89)
>
> Soman admitted that the data had been fudged. He said he could not justify his actions, since he knew them to be wrong... "My actions," Soman told an auditor, "were done in the midst of significant pressure to publish these data as fast as possible so as to obtain priority" (Broad 1980a:41);

(3) justificatory accounting--a form of account-giving whereby the person denies the wrongfulness of his actions and accepts responsibility for them (Scott & Lyman 1968). These accounting strategies may entail appeals to a prior time or appeals to reason to justify the behavior in question. Concerning an experiment of untreated syphilis in Black men:

> When the story broke in 1972, many of those involved said attitudes toward race and human experimentation were different in the 30's; others said they were just following orders. (Strouse 1981:63, a book review of Bad Blood by J. Jones 1981)

During a congressional investigation of alleged violations of patients' rights:

> Freireich replied that federal regulations were becoming overly burdensome and are hampering scientific progress... "These regulations are in fact harming the very patients they are designed to protect"... He argued that consent forms are frightening cancer patients by disclosing the "intimidating details" of their treatment (Sun 1981:889); and

(4) evasion tactics--scientists, like others, may try to make themselves unavailable when reports of deviance are revealed. They may take a convenient "leave of absence" or stealthily retreat to their laboratories (see Culliton 1974; McKean 1981).

In sum, the stereotypic response of some scientists to public reports of deviance in science may exasperate the problem since it appears deviant from the ideals of disinterestedness, objectivity and neutrality. Also, it is remarkably similar to the stereotypic response of other deviants in society (Scott & Lyman 1968; Sykes & Matza 1957).

Another threat to the public support of science arises out of the increasing tendency to politicize and commercialize science at all levels in modern society (Haberer 1972; Mazur 1981). Science has become increasingly directed by market concerns, and in a very real sense, "science is for sale" (Barnett 1980:8). Science has been transformed from the socio-historical ideal of our culture to a marketplace reality, and this transformation may be directly related to both support for science and occurrences of deviance in science.

The politicized and commercialized aspects of science are evident in the increasing employment of research and applied scientists in justificatory activities. Different individuals, groups, and organizations in society retain scientists to legitimate their personal claims and vested interests, and scientists have willingly cooperated in order to advance their careers and to insure their livelihoods. We can, of course, see many examples of this transformation of science from an activity that only involves the search for truth to an activity which also involves the attempt to legitimate, justify and control. For instance, scientists of all kinds have increasingly assumed an adversary role in our post-industrial society (Jasanoff & Nelkin 1981; Gwynne, Malamud, Hager & Begley 1979; Mazur 1981). The preceding is evident when entomologists present contradicting scientific evidence on the safety of particular pesticides (Smith 1982a), or when competing scientific testimony is debated concerning environmental impact studies. This may range from studies of toxic wastes (Smith 1982b) to building nuclear power plants (Mazur 1981). Nor is this situation uncommon for the medical profession as well. We see this most frequently when drug companies and the FDA square off with opposing testimony concerning the value and safety of new drugs (Sun 1982a). Military scientists have also assumed an adversary role as they make continued demands before congressional committees and the public for increased budgets to insure new advances in nuclear technology (Barnaby 1981). In fact, in recent years, we have all heard conflicting scientific testimony in areas of health, environment, ABM, and nuclear power, etc., which is difficult to attribute entirely to different scientific understandings.

But even more alarming are those overt attempts to politicize science. Marshall (1982) has reported that the U.S. Department of Agriculture is presently screening scientists "for security risks and political compatibility before inviting them to sit on peer review panels" (p. 600). And Culliton (1982) reports of the four scientists named to the President's Commission for the Study of Ethical Problems in Medicine, Biomedical and Behavioral Research:

> The fact that the four new Reagan appointees appear to have been chosen as much for their Republican connections as for their scientific qualifications has also raised apprehension that the newcomers will try to infuse the deliberation with stereotypically "conservative Republican" views. (p. 715)

The important point to recognize is that science is being increasingly relied upon to justify and to "sell" particular programs and priorities. More and more, science assumes an adversary role where scientists are found debating their scientific facts. As a consequence of science assuming this adversary and justificatory role in contemporary society, the objectivity of some scientists is being questioned with the recognition of the relativity, and even vested interests, of many purported scientific facts. The willingness of some scientists to provide contradictory expert testimony in adversary situations may have enhanced their personal careers, but simultaneously compromised their scientific credibility. Putting science "up for sale" creates a situation where values and money become chief factors in the determination of scientific evidence and not scientific disinterestedness and objectivity. Using science to justify rather than to understand, subsequently insures a greater concern for and discussion of deviance in science. We should not be surprised, then, with an increase in the number of future public allegations of deviance in science, nor should we be surprised with the gradual waning of public support for science (Walsh 1982b) with the increase in its adversary role.

SUMMARY AND CONCLUSION

The present paper has treated science as a vital national resource, and deviance in science has been treated as the depletion and exploitation of that resource. Given the centrality of science in our everyday lives, scientific deviance at the interface of science with other institutions and societies can clearly result in devastating and even incalculable costs. For instance, millions of dollars may be wasted on fraud, misappropriation, and self-serving scientific projects, and science may be literally set behind by decades because of misdirected priorities and/or the loss of funding. The advancement of science may be impeded when commercial secrets take precedence over the free flow of information. Thousands of potentially skilled technicians, educators and scientists may be lost due to a miseducation which is based on or related to biased and/or fraudulent scientific evidence. National defense may be threatened by undermining support from the scientific community or by misrepresenting and self-serving scientific projects. The Third World may be further exploited and alienated, and U.S. society itself may become cynical and less supportive at a time when science has the greatest need for public support. It is probably for reasons like the above that Senator Edward Kennedy has said in relation to FDA rules:

> Inaccurate science, sloppy science, fraudulent science--these are the greatest threats to the health and safety of the American people. Whether the science is wrong because of clerical error, or because of poor technique, or because of negligence, is less important than the fact that it is wrong... (quoted in Smith 1977:1227)

When considered in this light, it is not so much the number of reported instances of scientific deviance per se as it is the long range consequences and/or the magnitude of negative effects resulting from certain forms of scientific deviance that should concern us most.

Scientific deviance along the external frontiers of science tends to undermine both the effectiveness of and the support for science.

The present paper has also introduced the ideas of *deviance in applied science* and *deviance against science* into the general discussion of scientific deviance. Interestingly, deviance in applied science has tended to be ignored or overlooked during the recent spate of reported instances of scientific deviance. Although applied scientists are more concerned with means/ends relationships than with cause and effect relationships, they should be equally committed to the ideals of science. However, the tendency to focus upon deviance in basic research may tell us something about contemporary concerns. It may be that by limiting discussions of deviance to the realm of discovery, we can safely downplay and minimize the extent of deviance in science.

We might also do well to consider how deviance in science may be related to deviance against science. The goal of excellence in science should make us equally concerned with protecting science from both internal and external sources of deviance. While not a well researched area, we can safely assume that deviance in science and deviance against science are closely related to one another. We suspect a kind of bi-directionality of influence between these two types of deviance. Deviance in science probably increases the likelihood of deviance against science, and deviance against science, in turn, probably increases the likelihood of deviance in science. For instance, during times of retrenchment, reports of deviance in science (e.g., the fabrication of data, misappropriation of funds, plagiarism, sabotage, etc.) may actually stimulate ideological attacks against science (e.g., attempts to reduce funding of the social sciences because they tend to threaten traditional values, beliefs and even vested interests). Similarly, deviance against science (e.g., attacks by laissez-faire ideologies to restrict the form of the scientific community or by creationist scientists) may limit the opportunity structure of science and/or perpetuate certain vested interests and result in the increase of data fabrication/fraud by some scientists bent on getting ahead and advancing their professional careers.

Proposed Solutions

The complexity of deviance in science is obvious and will necessarily require long-range planning and complex solutions. There will be no quick solutions. But we should be moving, nonetheless, in the general direction of implementing certain *structural changes* to address the social problem of deviance in science. Some of these solutions should include: (1) specific procedures and safeguards within the scientific community (see Barber 1982, and printed herein) and its external frontiers to minimize and control scientific deviance; (2) the elimination of the informal legal exemption enjoyed by scientists at the present time; and (3) a long-term national commitment to expand the opportunity structure of science and a legislated-by-law science budget.

There are a number of specific procedures and safeguards, for example, which could be implemented within the scientific community to reduce and control deviance. Most involve a change in professional socialization and the implementation of specific procedures/guidelines for the conduct of scientific research. These might include: providing training in ethics at the graduate level, the implementation of specific data investigative and data protection policies and procedures, a viable NIH policy on deviance in science, the creation of hotlines and the protection of "whistleblowers", and encouraging scientists to systematically investigate scientific deviance as well as to disseminate those findings to regulatory agencies, committees, etc.

Although each of the just listed procedures has its own merits, some of which are more immediate than others (e.g., establishing new safeguards), we believe the most important in terms of long-term payoffs for the entire scientific community concerns the implementation of a program of mandatory training in ethics. Science clearly has the capacity for both good and evil. Given the highly competitive, get ahead culture we all share, it would seem imperative that science and ethics should stand side by side. Training in ethics should be seriously taught along with theory and methods at the graduate level. This is, perhaps, the only way to insure that tomorrow's scientists will know for sure the difference between right and wrong before they consider particular actions and not afterwards as a consequence of having been caught. Also, this may be a realistic way to insure that science can remain justifiably self-regulating.

Concerning the present informal legal exemption enjoyed by scientists, there are a number of disconcerting parallels with other high status professionals in contemporary society. Scientists who commit certain acts (e.g., fraud, misappropriation of funds, sabotage, plagiarism, etc.) in the course of their professions are clearly engaging in white-collar crime--in relation to either state, federal or international law--as per the definition of white-collar crime (Sutherland 1940). Similar to other white-collar criminals, they are generally shielded by their special value to society and their high status from criminal prosecution. And similar to other white-collar criminals in other professions, they have learned that token enforcement is the rule. However, there is some evidence indicating that scientists who engage in white-collar crime may be even more immune from public surveillance, scrutiny and legal sanctions than their counterparts in other professions and occupations (i.e., business and government). When was the last time a scientist was personally fined for his criminal activity, let alone sent to prison? In truth, prosecution and conviction are rare and are only likely to occur for certain infractions (see Dong 1982, and printed herein).

The problem of white-collar crime in the scientific community is simply part of the larger problem of white-collar crime in general. Sanctions for white-collar criminals in contemporary society are much less severe than for other members of society who commit the same or similar offenses. Scientists who commit white-collar crimes, like other white-collar criminals, are literally above the law. For instance, if welfare recipients misrepresent their assets or exaggerate their number of dependents they will most likely be prosecuted and sent to jail. Yet, if scientists misrepresent the value or purpose of their research to obtain a federal grant, sometimes amounting to hundreds of thousands or even millions of dollars, they can rest assured they will not be prosecuted in the same way that welfare cheats will be. Where is the difference? Actually the white-collar crime of the scientist results in higher costs to society in terms of overall wasted expenditures, loss of trust and confidence in the moral fabric of society, and in creating a dismal sense of social inequity.

Based on the preceding, we believe white-collar crimes in science or in any other profession should not go unpunished. Scientists who engage in different forms of white-collar crime in the course of their professions should be held legally responsible for their actions, like anyone else in American society. This should not be misconstrued to imply that we should distrust the scientific community. Rather, we should simply recognize that opportunities for deviance and crime exist within science, as they exist in other professions and occupations in contemporary society, and should be realistically assessed, anticipated and fairly dealt with.

Third, and lastly, there is the pressing need in the U.S. to significantly expand the opportunity structure of science. The immediate effect of which would be to attract more talented young people into the scientific professions, to increase the probability of favorable returns to society for its investment in science, and to help ameliorate many of the existing pressures (e.g., excessive competition, careerism, differential opportunities and favoritism, etc.) that are conducive of scientific deviance. There is a national need for both an increased science budget and a steady science budget; that is, a science budget which would be legislated by law (see recent efforts by France in this regard, Walsh 1982a). Such an undertaking would increase the likelihood of moving science beyond those conditions that foster scientific deviance as well as moving science beyond the political whims of changing administraticns. It would also bring us a step closer toward achieving both the progressive advancement of science and a science-benefiting society.

ACKNOWLEDGMENTS

We are especially indebted to the helpful comments and suggestions made by Edward Sampson and Robert Winzeler.

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PART FIVE

The Larger Issue: Deviance of Science

Part Five proposes a larger perspective by which the research enterprise can be viewed. It looks at the rules of science, offers a critical perspective, and suggests some ways that we might play the game by a different set of rules. In Chapter 10, Sampson links scientific racism, classism, and sexism to the hierarchical-instrumental thinking style of scientists. He warns us of the deviance of science and offers an alternative, more gentler attitude for our consideration. Reed similarly attacks the assumptive base of science in Chapter 11, but he does so in relation to economic rather than social theory. Economic theory is viewed as deviant from the ideals of science and deviant from its intended purpose of facilitating our understanding of economic behavior. Finally, in Chapter 12, Dittmann discusses the normative deviance of science (i.e., its egregious misuse in the name of militarism and poverization) and how it depends on a particular ideology of science and a particular philosophical foundation. Examples from the nuclear power debate are provided.

THE INVERSION OF MASTERY[1]

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Let me frame the following paper between two contrasting themes, one reflecting the tasks of mastery, the other its inevitable inversion when it alone dominates. The first theme was presented most crisply in the late 1500s and early 1600s by Sir Francis Bacon. He both captured the spirit of the Western worldview and simultaneously set the tasks for which we are still engaging the most substantial investments of our world and human resources:

> the sovereignty of man lieth hid in knowledge...now we govern nature in opinions...we should command her by action...knowledge that tendeth but to satisfaction, is but as a courtesan, which is for pleasure, and not for fruit or generation...the true end...of knowledge...[does not lie] in discourse...or...arguments, but in effecting and working... (from Horkheimer & Adorno 1972, pp. 3,4,5)

This Baconian view of knowledge should be familiar to all of us. We are guided by it in our daily practice. Even psychology's research affirms mastery as a central motive in all human life, perhaps as fundamental to human survival as the very air we breathe. As Bertrand Russell (1945) noted, to Bacon goes the credit for joining knowledge with power, control and domination. Through science and technology, man would come to dominate the unruly forces of Nature and so clearly establish his sovereignty.

The second theme emerged in the Western world most clearly in the writings of the group known as the Critical Theorists of the Frankfurt School of Social Research, including Max Horkheimer (1972, 1974), Theodor Adorno (1973), Herbert Marcuse (1964, 1977) and more recently Jurgen Habermas (1971, 1973). This theme has similarly become a central element in several more recent feminist (e.g., Merchant 1980; Spretnak 1982) and ecologist (e.g., Berman 1981; Bookchin 1982) publications.

Reflecting on the Baconian view of knowledge as mastery and as the driving force behind the age of Enlightenment and the rapid rise of modern Western science, Horkheimer and Adorno write of the self-destructing patriarchy that worldview represents. Even Bacon's choice of terms reveals the patriarchy that lies at the center of this notion: nature is female, even a courtesan; she is to be ruled by the men of science and technology who will wrest her secrets from her in order to demonstrate their own sovereignty.

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[1] An initial version of this paper was prepared while the author was on leave from Clark University and affiliated with The Saybrook Institute.

Writing their massive joint work, Dialectic of Enlightenment in 1944, Horkheimer and Adorno's opening sentence outlines their concern and their project:

> the Enlightenment has always aimed at liberating men from fear and establishing their sovereignty. Yet the fully enlightened earth radiates disaster triumphant. (p. 3)

Somehow, the hope that the knowledge gained in order to achieve mastery would free humanity from its era of superstition and enslavement became inverted: the massive accumulation of such knowledge only further entrapped humanity. Listen to Horkheimer (1974):

> If by enlightenment and intellectual progress we mean the freeing of man from superstitious belief in evil forces, in demons and fairies, in blind fate--in short, the emancipation from fear--then denunciation of what is currently called reason is the greatest service reason can render. (p. 187)

In these few passages, Horkheimer and Adorno describe what can be referred to as the inversion of mastery. The tasks of mastery over nature demand a kind of patriarchical and commanding relationship between people and nature that fosters an instrumentalized process of reasoning and thinking; that is, reasoning is employed as an instrument or tool designed to accomplish the ends of greater control over a particular phenomenon. As that process dominates humanity and comes to define and organize the entirety of human knowledge, we see the inversion from mastery into submission.

Everything is defined in terms of technique and control, only technically utilizable knowledge counts. As problems mount, they are addressed in terms of the same instrumentalized reasoning that produced the problems in the first place. As we will see, the remedy has become the poison; as its dosage is increased, the patient becomes even sicker.

Instrumental Reason

The key to the Critical Theorists understanding of the transformation of knowledge from remedy to poison is based on the notion of instrumental reason. Horkheimer defined this type of means-oriented reasoning as being primarily concerned with "the adequacy of procedures for purposes more or less taken for granted and supposedly self-explanatory" (Horkheimer 1974:3). To think instrumentally is to be concerned with the coordination of means to ends, not with the ends themselves.

A clear example of this notion is presented by the economic historian, Karl Polyani (1957):

> Rational action is here defined as choice of means in relation to ends. Means are anything appropriate to serve the end, whether by virtue of the laws of nature or...the

> laws of the game. Thus "rational" does not refer either to ends or to means, but rather to the relating of means to ends. It is not assumed, for instance, that it is more rational to wish to live than to wish to die... For whatever the end, it is rational to choose one's means accordingly.... Thus it is rational for the suicide to select means that will accomplish his death. (pp. 245-246)

The argument mounted by Horkheimer, Adorno, Marcuse and Habermas suggests that the definition of rationality in terms of instrumentality--derivative from the Baconian directive--limits understanding to forms that coordinate means to ends. It stresses the notion that facts exist as neutral attributes of the world to be used either for good or for evil. The latter, however, because they involve questions of value and preference, lay beyond the purview of science which deals only with "what is," not with "what ought to be."

How facts will be employed, in whose interests and towards which ends, thereby involves questions said to be beyond proper science. However, as the Critical Theorists note, the facts that are gathered under this instrumentalized frame are hardly disinterested and neutral; they are guided by the patriarchical-instrumental-mastery interest in knowledge. A fact is a fact only insofar as it pertains to something that is technically utilizable, in the service of mastery and control. The only legitimate forms of knowledge become those which are geared to achieve such ends. Yet, these ends are deleted from critical reflection and so creep in as an unnoticed bias or interest.

Basically, a partriarchical perspective wins in the guise of a value-neutrality that rules out other interests except its own, which, however, it fails to recognize in the first place. Mastery as the remedy becomes the poison. In ruling out other ends besides its unrecognized-own, all that is presumed in its terms to be unruly in nature must be dominated. And, any organism that destroys what it takes to be its other, not recognizing itself in that other, lays a firm foundation for self-destruction. This latter point is developed more fully in later sections of this paper.

A Gentler Attitude

Before leaving this overview of the contrasting themes that mark the boundaries of my discussion, let me briefly quote a passage buried in the middle of a Psychology Today (Gardner 1981) interview with Howard Gruber, a psychologist whose recent works have provided an insightful understanding of the life experiences of several creative scientists. Gruber was attempting to answer a question about why some societies (e.g., Japan) seem to produce so few Nobel Laureates, while others dominate. Gruber suggested that the answer might lie in a different view of the relationship between people and nature:

> Suppose you have a view of man as a participant in nature rather than a master of it, the usual Western notion. For the intellectual tasks we've had up to now, that may not be a very fruitful approach. But I leave open the possibility

> that a gentler attitude towards nature may turn out to be more illuminating and more fruitful in meeting our future needs. (p. 70)

This is an intriguing statement. On the one hand, it affirms the Baconian view of man as the master over nature, at least for one period of Western history. On the other, it reflects an alternative possibility suggested as well by the Frankfurt theorists: namely, what once worked so well has now turned about to threaten. Another relationship between humanity and nature may be essential in the years ahead, what Gruber terms, a gentler attitude.

A Radical Deconstruction

So that the dimensions of my commentary can be reasonably clear at the outset, let me briefly observe my own understanding of the deconstruction of mastery that is called for. A major consequence of the Enlightenment was the transformation of the source of the authority for knowledge from the Church to empirical science. This change in source, however, failed to change the form that subsequently developed (see Sampson 1978, 1981). The worldview of the Church was patriarchical. The Enlightenment, while challenging the Church's authority and replacing it with the rationality of empirical science, failed to transform that patriarchical hierarchy. The improvement was substantial and not to be slighted; yet it did not undermine the mastery theme that has marked Western religion and Western science as a fundamental element in its worldview.

Deconstructing that hierarchy does not entail substituting another in its place. Rather, any meaningful transformation must necessarily get to the very heart of the matter itself: hierarchies and privileging must be set aside to be replaced by views of knowledge that recognize differences but in no way demand that one form be declared the winner over others.

For example, the dominance of a patriarchical framework threatens survival as its inversion undoes whatever benefits flow from the concept of progress-as-control that it advocates (see Bookchin 1982; Schell 1982; and Wilden 1980 for several arguments that develop this point). And yet, when that frame is cast aside completely, survival and well-being are likewise threatened. The challenge then is not to replace the mastery theme by some purely gentle and receptive outlook, but to generate structures within which dominance hierarchies are absent and various interests gain an equal hearing in practice.

Having said that, let me also observe that those of us who are concerned with instituting such a change operate within an already dominant hierarchy. The press for an alternative will inevitably appear to be contrary to what I have just said. The quality of argumentation emerges whenever one must employ the framework to be undone in its undoing. This aim is not to repeat past history in which sources changed but patriarchical forms were retained, even if the arguments on behalf of a change appear to be cloaked in that one-dimensional rhetoric.

Deviance In and Of Science

Now, what does all of this have to do with deviance in and of science? A great deal. I have been describing the transformation of a remedy into a poison. I suggest that the passion for patriarchical mastery not only drives the deviant behaviors within science (e.g., fraudulent reporting of data) but also the deviance that the narrowed and unexamined tenets of science-as-scientism has become. The "in" and the "of" do not refer to two independent events, but rather emerge from the same worldview and underlying social system within which science itself dwells.

The first, deviance within science, describes a pursuit of winning at any cost: govern and control with an iron hand even if that requires distorting and suppressing the unruly moments that inevitably appear. Several papers in this collection provide more than ample testimony to this kind of deviance and its costs. The second, the deviance that is of science, represents a fundamental distortion in values that occurs when only one framework becomes valid for defining knowledge and cannot be applied critically to uncover the values that undergird and support that framework itself.

So that this will not be an excessively abstract discussion, I will call upon three examples from the human sciences that I believe illustrate what I mean when I speak of the inversion of mastery. These examples reveal the combined effect of deviance in and of science. The patriarchical perspective is apparent in these examples in several senses of that term: not only is the attitude one of mastery but in addition it is applied specifically in order to achieve a kind of patriarchical mastery over those groups and persons who have symbolized nature or its potentially unruly qualities.

The first example involves sexual bias, the second, racist bias, the third, class bias. There are good reasons for a patriarchically governed science to be driven to distort or destroy those elements of the human world that challenge the hegemony of the patriarchical-instrumentalized worldview. Each case example illustrates the profound meaning of deviance in and of science when the illusion of objectivity blinds those who use it to achieve the culture's implicit goals as though the "facts" in each case were self-evident.

Sex Bias. By the 19th Century, some key philosophers had laid a clear foundation, seconded heartily by the patriarchical religion of the Judeo-Christian tradition, for the belief that women were inferior to men (see Shields 1975 for the basis for much of the material in this section; also see Merchant 1980). It was up to the scientists of the time to conduct systematic research that would demonstrate the objective facts of the case. What passed for science in the late 1800s, however, seemed more often like a circus sideshow than science as we now know it. Nevertheless, the major phrenologists of the time, Gall and Spurzheim, reasoned that if the male and female bodies differed, then surely the male and female brains should likewise differ. In particular, those parts of the brain that involve higher thought processes should have a greater and more visible development in men than in women; the latter should show greater development in areas said to be involved with feelings and emotions.

It was not difficult to find affirming results by taking readings of the bumps on the heads of men and women. But it awaited more stringent scientific tests, especially those involved in brain weight and brain localization, to put the matter to what would seem a more proper test. Several investigators concluded that the male superiority appeared most clearly in a weightier brain. Others performed careful dissections and discovered that the male brain was more developed in the frontal regions--where higher thought processes were presumed to be located--while the female brain was parietally dominant--where more primitive functions were served. Let me note, only parenthetically, that when still later investigators suggested the key role of the parietal regions in higher thought processes, it was necessary to rethink the earlier findings showing female parietal dominance so that they would be congruent with the belief that males were dominant in those regions responsible for higher thought.

As Shields (1975) observes, this change required some dramatic revisionism as revealed in the following quote from 1895:

> the frontal region is not, as has been supposed small in woman...but the parietal lobe is somewhat smaller...a preponderance of the frontal region does not imply intellectual superiority...the parietal region is really the more important (from Shields p. 741; originally quoted by Patrick 1895).

This is quite a revision considering that in 1854 a neuroanatomist had concluded that women are *homo parietalis* while men are *homo frontalis* in order to affirm male dominance in higher thought.

I hasten to note that these earlier investigations did not simply go away with the passage of time or with the sophistication of new methodological techniques; they remain in modern garb with us today. Many men of science still work diligently to demonstrate that the culturally and historically constituted differences between men and women are *entirely* linked to differential brain structures.

Thinking and Reasoning. One need not probe brain neurophysiology to uncover scientific work that in its own way affirms the superiority of the male worldview. Far more subtle efforts have recently emerged within the psychological study of reasoning processes, moral reasoning in particular (e.g., Kohlberg 1968, 1969).

Suppose that one could uncover a sequence of cognitive development that moves from a more primitive (should we say, nature-like?) to a more advanced form (shall we say, un-natural?). Suppose further that the primitive form is said to reason in terms of concrete human relationships, whereas the advance form reasons by means of abstract and formal-logical principles. And finally, suppose that the determination of a person's standing along this developmental scale systematically scores thinking that reasons in terms of human relationships and human connections--often given by females--as lesser than thinking that is abstract and formal--of the type more often given by males (e.g., Gilligan 1977).

What is so subtle about this form of bias is that it adopts its own framework as the model of higher thought and then uses that model to affirm itself while placing all other forms at inferior positions along a developmental scale. The rules by which the game will be played are set. The key feature of these rules is that whenever they are employed, one side will necessarily be declared the winner of the game. Furthermore, the rules also set the standards by which any other rules are to be evaluated. Finally, the same rules implicitly deny as relevant any values other than those involved with technical mastery and control. Yet, this denial occurs without being recognized as such. All of this provides a surefire way to prove the prowess of one side and the inferiority of the other as long as the rules can never be seriously questioned.

What I have just described is perhaps the major theory and program of research and practice on reasoning and morality that exists today. This is a program that is being taught in schools to teachers who will then carry its message to their students; it is even being used to instruct various groups on moral behavior. Elsewhere, I have argued that the so-called highest stage of moral reasoning corresponds not coincidentally to the patriarchical worldview (see Sampson 1977, 1978); it implicitly adopts this view as the standard by which to measure and evaluate all other forms. Thus, it employs hierarchy rules, yet does so as though it were objective and hence natural and neutral, a mere description of the world as it is.

While I believe that the main point is by now reasonably apparent, many additional examples of this patriarchical worldview have been recently noted. For example: (a) the sociologist Jessie Bernard's (1982) recent commentary on the patriarchical versus a contrasting view of pre-human primate life; (b) the historian Gerda Lerner's comment (Hook 1981) on the periodization of history in terms of patriarchical categories of war, conquest and domination; and (c) Merchant's (1980) excellent and detailed analysis of the other side of the Scientific Revolution and its notion of progress.

Socially and historically, the world of the female has represented the world of nature. This describes a world that in the last several centuries, during which time industrialization, widespread capitalism and scientism grew enormously and held sway over the lives and the imagination of the Western world, had to be dominated, tamed and ruled. Indeed, the deviance both in and of science, in my view, offers reams of testimony to the distortions and self-destructing quality that has emerged as the patriarchical worldview has been relentlessly pursued throughout the history, both past and present, of Western Civilization.

Immigrants and Racist Bias. At the turn of the century, the United States witnessed substantial immigration from European nations. Although these immigrants were both eagerly sought in order to work the developing factories and to populate the still sparsely settled land, they were also feared for their differences and their potential pollution of the good American stock. Years earlier, Francis Galton, for example, had suggested an innate intellectual inferiority among the races, especially Blacks (Samelson, Note 1). Needless to say, when the first white settlers (the Puritans) were the actual immigrants to the United States, they sustained racist attitudes towards the Native Americans they encountered. However, they employed religious rhetoric rather than intelligence testing to affirm their beliefs that they, the white settlers, were the chosen people. The existing native populations were said to be the inferiors to be tamed, the "villains in a sacred drama" to be conquered and destroyed (see Segal & Stineback 1977).

In the early years of the 20th Century (1910s-1920s), as immigration substantially increased, Congress sought to limit the influx, but especially of certain types from certain nations. They found ample support among several prominent social scientists for the belief in a basic inferiority of certain immigrant groups. For instance, in 1913, Henry Goddard was invited by the U.S. Public Health Service to go to Ellis Island and to administer the Binet-Simon intelligence test to samples of arriving immigrants (see Bem 1975). His data revealed that 83% of the Jews, 80% of the Hungarians, 79% of the Italians and 87% of the Russians were clearly feebleminded (see Dorfman 1982; Kamin 1974, 1982 on these findings). However, Goddard saw this population of feebleminded to be potentially useful because:

> these immigrants...do a great deal of work that no one else will do... It is...true that there is an immense amount of drudgery to be done...work for which we do not wish to pay enough to secure more intelligent workers. (Kamin 1982:98)

But, Goddard held little fear for the children of such persons, reasoning that their feeblemindedness was probably not a hereditary defect.

In 1923, Carl Brigham observed that those who had immigrated to the United States 20 years earlier were as bright as native born Americans, although this was not the case for those who had immigrated to the United States during the last five years. He argued that this difference had less to do with acculturation than with race: the former groups were from Nordic and English stock, while the latter were Mediterranean or Eastern European.

The implications of these analyses are as clear today as they were at the time they were developed; they also parallel contemporary efforts by Jensen and others (see Hirsch 1981 for a well documented, critical analysis of these current endeavors). Even if we leave aside the distortions in fact and in interpretation that run rampant throughout most of this work, and even if we leave aside the many serious questions raised concerning the honesty in data collection and reporting with which some aspects of this early work was undertaken (e.g., the Cyril Burt controversy), we have an important ideological use made of a particular testing tool.

The I.Q. test was not invented simply to understand people but to arrange, classify and apportion them in ways to justify, legitimate and often conceal the technological demands of the developing machine-society. As two critics, Bowles and Gintis (1973) comment:

> I.Q. is not an important cause of economic success; nor is the inheritance of I.Q. the reason why rich kids grow up to be rich and poor kids tend to stay poor. The intense debate on the heritability of I.Q. is thus largely irrelevant to an understanding of poverty, wealth, and inequality of opportunity in the United States (p. 2)

The passion to control people, in this case immigrant populations, motivates the distortions that I have termed the inversion of mastery.

Industrial Relations: Hawthorne and Class Bias. In the 1920s and 1930s, driven by a concern with issues of worker productivity within the burgeoning factory system, a group of Harvard investigators entered into a research program at the Hawthorne plant of the Western Electric Company (e.g., Mayo 1933; Roethlisberger & Dickson 1939). Ostensibly designed to examine the effects of certain physical factors on worker productivity (e.g., lighting, rest breaks to reduce fatigue), the investigators purportedly uncovered the then surprising finding, termed the Hawthorne Effect. Briefly, these investigators found that whatever management did to the workers, raising lighting or lowering it, increasing rest periods or decreasing them, the workers' productivity appeared to increase. This effect was subsequently attributed to the attention that management showed the workers. Even today, numerous texts carry this as the basic message of the Hawthorne studies: workers are motivated by being shown attention from management. Indeed, one is led to believe that attention overrides such mundane factors as pay and working conditions!

While numerous criticisms of the Hawthorne program emerged over the years, Bramel and Friend (1981) have recently set the record straight by uncovering the self-deceptions of the original investigators. Let me cite only a couple of the many examples they present.

When it became apparent during the study that several workers were not taking as kindly to managerial interventions as others, a decision was made to terminate two unruly workers and to replace them with two more cooperative workers; that is, two who would produce regardless of the work conditions presented to them. To find an increase in productivity after replacing two recalcitrant workers with two cooperative workers, and then to attribute this increase to benevolent management, seems not only to miss the point but to manufacture the point that the researchers and the management were hoping to find in the first place.

Bramel and Friend report another related instance involving the interpretation that the Hawthorne investigators made of the resistance they encountered from one particular worker. In writing to their benefactors at the granting agency, the Rockefeller Memorial, these investigators observed that a woman had been dropped from the study because she had "gone Bolshevik." As though that were not sufficient, the investigators added that it was later discovered that she had been sick with anemia at the time of her reported noncooperation, and that "After treatment for the anemic condition...in subsequent discussions

[she] disavowed her former criticism of the company" (Bramel & Friend 1981:871).

In other words, according to the Hawthorne investigators, workers who refused to cooperate could not do so for rational reasons, but only for irrational political motivations or because of ravages of illness that had robbed them of all reason. In this particular instance, the scientific investigators had simply assumed that worker-management conflict must be nonrational since both groups must surely share the same interests.

SUMMARY

I selected three different though related cases in order to illustrate both deviance within science and the deviance of science. In each case, the patriarchical pursuit of mastery pressed the investigators to distort their findings without significant awareness of that distortion. This resulted in subsequent generations of scientific investigators accepting many of these findings without question. After all, the facts seem to speak for themselves; and furthermore, the facts are congruent with the patriarchical worldview from which they were generated and in whose name they have functioned.

In noting this generational transmission of scientistic ideology, I am agreeing with the key point recently made by Samelson (1980). He questioned the classical research study reported by J. B. Watson that founded behaviorism in psychology, as well as Cyril Burt's by now infamous twin studies. More significantly, however, Samelson has challenged those who have accepted these findings without critical doubt: "it took more than two isolated individuals to give their data, for considerable time, an evidentiary status they did not deserve" (Samelson 1980:623).

The deviance of which I speak, therefore, resides not in the individual or even the group of individuals who conduct scientific work in the first place. Rather, the deviance within and of science resides in the entire cultural system that employs a single, Baconian framework which necessitates these kinds of distortions. The solution to the problems raised is not more of the same, but the development of significant alternatives to the patriarchical-dominant worldview that has thus far dominated our Western understanding and practice.

So that my point is not misunderstood, I am not using these examples to describe mere errors in judgment or slippages of reason. In my view, they represent the fundamental inversion of mastery, the ideological distortions that are generated when instrumental and technical reason become the absolute judge and jury of all human knowledge, and produce what Habermas (1971) terms scientism:

> the conviction that we can no longer understand science as one form of possible knowledge, but rather must identify knowledge with science. (p. 4)

That knowledge form represents the patriarchical attitude, the sovereignty of man over nature and all that symbolizes nature.

Conclusion: Towards an Alternative

I have suggested that in the single-minded pursuit of mastery, the pursuer becomes the pursued, trapped by the very lures and snares established to catch and dominate the presumed "enemy." The very tools and institutions established in the first place to achieve mastery become the source of the new problems that humanity confronts (see Ogilvy 1979 on a similar point). What is called for, therefore, is a different relationship between humanity and nature, one that partakes less of mastery and more of participation and receptivity. To paraphrase Gregory Bateson (1972), those who seek to control their environment by exercising their sovereignty over it, as though they and it were not part of the same system, find themselves destroying the very other that they need for their own survival.

The gentler attitude suggested by Gruber describes an approach that emphasizes more receptivity, connectedness and participation than conquest. Unfortunately, the very social institutions that feed upon mastery, and here I refer to the underlying socio-economic structures, especially advanced capitalism, that have come to dominate the Western worldview and human consciousness, seem to demand the poison that passes for the remedy.

Let me focus this point by calling upon a fascinating discussion which I first encountered in a paper by William Leiss (1975) in which he cites the ideas of Lawrence Tribe, a Harvard law professor, and the works of Peter Singer and Christopher Stone. Tribe speaks of constitutive rationality to describe an alternative to the means-ends rationality of instrumental reason. He argues that technological decisions are not merely to be understood in terms of some payoff matrix with seemingly factual entries being made and some equation solved. Each technological choice transforms the experiences and the consciousness of the individual and the community. Thus, more than mere instrumentality must enter into the very knowledge-constituting processes.

In other words, given the profound impact of instrumental acts on human life and well-being, bases of knowledge beyond the instrumental must enter as vital components of what is held in high esteem within a culture. This alternative of constitutive rationality can be used then to supplement the existing dominance of instrumental rationality. Tribe seems to be describing an attitude beyond mastery, a more receptive relationship between humanity and nature than the patriarchical view that has thus far so heavily dominated and threatened our civilization: both by tearing it apart from within (as the three examples previously cited suggest), and by threatening it with environmental destruction (e.g., Schell 1982).

Both Singer, on behalf of animals, and Stone, on behalf of all natural entities, suggest the need to grant legal standing to nature so that its interests can be given due recognition in all matters at hand. Stone urges us to grant forests and streams legal standing as entities with separate and important rights and interests of their own, lest in the name of human mastery, we view the only interests to be those of human sovereignty. We have recently seen several legal cases of this sort in which the rights of an endangered species to survive is represented in opposition to the rights of industry to build a new hydroelectric plant.

Several other cases also highlight this theme. In Canada, for example, this is revealed in the plight of Native groups to preserve their land rights (i.e., its sanctity) versus the rights of technological interests to transport gas from Prudhoe Bay across northern Alaska and the Yukon into the Mackenzie Delta and down into the United States (see Winter 1981). Although the Canadian government and several gas and oil interests in both the United States and Canada favored the pipeline, a commission was appointed with Justice Thomas R. Berger in charge to examine the social and environmental impact of the pipeline. Arguing that their case went beyond mere ideology as it was founded on clear-cut facts, the industrial side and its scientific advisors sought to further its interests in extending human mastery and sovereignty. The other side countered by attempting to reveal the gap that separated the presumed "logics" that are involved. Speaking on behalf of the Native groups, one representative observed:

> Somehow in your carpeted boardrooms, in your paneled office, you are plotting to take away from me the very centre of my existence.... Deep in the glass and concrete of your world you are stealing my soul, my spirit. By scheming to torture my land, you are torturing me. By plotting to invade my land, you are invading me. If you ever dig a trench through my land, you are cutting through me. (Winter 1981:101)

A very similar sentiment was also recently expressed# by the Hopi elders who saw their land likewise threatened by the onslaught of what passes for progress and the logic that governs its understanding (see Harris 1980).

Surely, the reconciliation of these competing paradigms, logics or modes of reasoning, cannot be accomplished by employing the terms and understanding of the mastery-oriented model to evaluate the challengers to its ascendency. There is no room within its ostensibly neutral but highly interested technological worldview for the imagery captured in the preceding quote. A greater balancing and legitimacy must be granted to those whose attitude towards nature is more receptive and open than the worldview that would simply divide and conquer.

A New Metaphor

There have been several calls recently for a changed metaphor to guide human life in the years ahead (e.g., Lakoff & Johnson 1980; Marcuse 1977; Merchant 1980; Ogilvy 1979; Sarbin 1977; Wilden 1980; Winter 1981). Some have countered the metaphor of mechanism, patriarchy and instrumental logic with a call to return to a more organic metaphor that grants domination to the spiritual world. Others have called for a third alternative, the artistic or aesthetic metaphor that recognizes the both and that comprises the ecosystems--humanity and nature--in which people are a part, but not the whole.

A new metaphor, strengthened and reaffirmed by structures that sustain and nourish it, surely is necessary lest we literally employ one view of progress to end all progress. I am not simply referring here to the nuclear arms race, but to all forms of destructive domination and exploitation which in the name of governance must defeat the governed and all those deemed to possess a kinship with nature. This involves the relationships between men and women, between whites and nonwhites as well as between those who presently possess economic dominance and those who remain part of the world's underclass.

Thus, the *inversion of mastery* describes a kind of deviance that extends well beyond questions of honesty in gathering, analyzing and reporting one's research findings. It pertains to the very manner by which those who seek understanding must do so from within the cultural and natural webs that join us all together as participants, not combatants.

Reference Note

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THE MYTHICAL CONTENT OF ECONOMIC THEORY

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The Presidential election of 1980 brought to popular attention a set of economic propositions known as supply side theory. Proclaimed as a solution to the problems of inflation and unemployment, the Reagan administration used supply side theory to restore the "natural" balance of markets by cutting social spending which had supposedly distorted normal market relationships. Widely touted as a new approach which would help stimulate growth, the theory was not significantly demystified until The Atlantic Monthly published William Greider's (1981) article on "The Education of David Stockman." In that article the biases of supply side theory were revealed and its artificial empirical base exposed when David Stockman was quoted as saying, "None of us really understands what's going on with these numbers" (Greider 1981:81).

The objective of this paper is to outline what is in fact a problem that is not unique to David Stockman: that the economic theory which produces his or any other numbers does not allow for a full comprehension of the world that produced those numbers.

Such a situation points to the uneven development of economics as it is practiced today. On the one hand, economists have created a powerful set of theoretical categories and economic models. On the other hand, they have become increasingly unable to translate their creations into meaningful terms by which they can comprehend "economic" behavior. It is this disjunction between the ideal and real worlds of economists which I will label as the deviance of economics.

There are two reasons for this uneven development of economics and its deviance. First, economists claim their discipline is a positive science which requires that they maintain a disinterested and independent stance (i.e., that their economics be value free). However, in reality, economists have denied the infusion of values into their scientific discipline, thus violating the ideals of scientific disinterestedness and objectivity. But such a utopian view of the philosophy of science is, of course, hardly a valid one since any line of inquiry requires a distinct set of presuppositions about what is and what is not worthy of scientific inquiry (Dwyer 1982). Second, the values which economists deny literally determine the nature of their "economic" facts. That is, the policy statements of economists necessarily reflect a particular ideologically-bounded view of the world, are often intertwined with "pre-scientific" thinking, and have tended to distort and/or obscure the socially constructed market forces they presume to be natural and universal. In other words, a certain kind of "economic" fact is socially and ideologically constructed using all the trappings and verbiage of contemporary science. The nature of supply side theory, for instance, and its "pre-scientific" content may in fact be seen not as neutral, but as royalist (Rousseas 1982), and predisposing of certain desired social relations and economic behavior in society.

The purpose of the present paper is to make more apparent this disjunction between the ideal and real worlds of economists, and hence elucidate the deviance of modern economics. The concepts of demand, supply and market are used as case examples and are discussed in relation to the social and political bases of contemporary economic theory.

Evolution of Economic Theory

The theory from which David Stockman deals, like all contemporary economic theory, can only address the contemporary since it ignores history. Interestingly, the core of his theory was developed in the latter part of the 19th Century in a series of paradigm changes labeled the "neoclassical revolution." Prior classical theory had dealt with aggregate questions revolving around the accumulation of commodities and their distribution via the market. Concerned primarily with issues of growth and social change, the classical theorists from Adam Smith to Karl Marx had focused on the interaction between men and the material world and had sought to discover "the methods of increasing the quantity and the physical productivity of resources" (Myint 1965:xii).

With the crisis of classical, competitive capitalism in the latter part of the 19th Century and the subsequent rise of a new theory, a different set of concerns rapidly developed. Economists focused their concern on the "equilibrium solutions" which a competitive economy produced rather than the growth of the total level of commodity production. In a presumably static ahistorical world, analysis moved to a new level at which a given quantity of commodities were to be distributed vis à vis the market in order to maximize individual and hence aggregate welfare at the level of individual perception (i.e., the subjective). An economist then, and now, assumes that "there is no future possibility of increasing economic welfare by a more efficient allocation of the given resources... The subjective welfare analysis is closely related to the 'scarcity concept of the economic problem'" (Myint 1965:xii).

Thus, by moving to the subjective level of analysis, economists adopted a theoretical outlook which relegated the material world to a set of secondary, derivative propositions which were fully consistent with the emergent positivist views of science (Kolakowski 1969). Economists henceforth assumed for scientific purposes that the economic element in society was not only separate but separable from the rest of society. Even more important, they abandoned the realism of classical economics and began to assume that the "economic" was constituted solely by subjective forces.

The discipline took on a very specific and unique character. "Economics, as we have seen, is concerned with that aspect of behavior which arises from the scarcity of means to achieve given ends... It follows that economics is entirely neutral between ends" (Robbins

1952:24). Yet, contrary to such assertions, modern economics does have a value-base. Instrumental reason, upon which economic theory is based, conforms to anything; it is able to attack and to defend the same position. Its value position is evident in its denial of an obdurate reality to which our thought must submit. Its ideological ramifications are evident in the discounting of any negative critique of itself in the name of affirming the plausibility of "what is" the status quo. Further, its value-laden character results from the fact that the world outside the economy is presumed to be given and fixed under the "ceteris paribus" status of scientific non-concern.

Modern economic theory thus presumes itself to be asocial, ahistorical and universal. But as a tool for analyzing social reality, the "new" scientific economics necessarily requires a fragmented and partial view of social reality; that is, the social world which produced it be divided at the individual level into objective and subjective components (Kosik 1976). Such a positivist framework legitimates the analytic neglect of an external and objective social reality independent of the individual so that economic science may construct a world based entirely on individual subjective states. Out of the perceptual world of the individual arises the categories of objectivity and subjectivity.

One consequence of this fragmented view of reality was that economists focused their attention on developing tools to analyze prices and quantities as observable phenomena capable of being measured, laying aside the question of their origin in the perceptions of individuals. By adopting this positivist stance, economists were able to accomplish two things: first, they were now capable of asking questions pertaining to the abstract, isolated categories of the "fictive" market; and second, to show that these categories would produce, within a consensus framework, a market equilibrium in the face of scarcity. With the corollary of unrestrained competition, it was possible to argue that not only a single economic situation, but the entire social fabric could now be defined with the same optimizing logic. Any departures from this position, of course, reduced social welfare and were therefore to be resisted. Most generally, this denial took the form of state manipulation.

A remarkable aspect of this emergent theory was its emphasis upon the idea of freedom, both in relation to the principles of self-interest and in the social theory of possessive individualism which denied the existence of autonomous social institutions (MacPherson 1970). While arguing from this base, however, the new economics had to establish the mechanisms under which the optimal conditions could be met. That is, in order to allow for the expression of perceived freedom, economists had to help in the establishment of techniques of social control.

This conjuncture of a new subjective theory and the related function of social control theory created the paradox and contradiction of modern economics (Silva & Slaughter 1980). The "solution" to that contradiction was positivist economic theory. While it allowed the development of a subjective economic system, an ideological plus for this nascent science and its masters, it also predisposed economics to an ego-centered, non-social theory of institutional functioning and individual deviation. It was this combination of subjective theory and individual concepts of deviance within the framework of socialized production that generated the present crisis and failure and which Stockman in his orthodoxy has confessed to.

I will argue that the introduction of positivist philosophy as a response to the "subjectivizations" of economics was therefore the basic problem which the discipline has yet to overcome. Working off the propositions of positivism, the tools of economics have become mathematically sophisticated and practically barren: they are able to assemble and incorporate large quantities of data but unable to explain them; they are able to construct typologies of market situations but unable to penetrate their social base; they can posit the small firm but not the growth of the corporation. In short, economics purports to analyze any number of situations on the basis of a few simple propositions which are increasingly anachronistic.

Economists approach the current situation by examining, in turn, the abstract notions of demand, supply, and their joining in the equally idealized market. In theory, these concepts are relatively simple ones. However, as explanatory tools they are frequently contradictory; in order to make sense they require the invoking of knowledge that lies outside the boundaries of the discipline. Yet, in the face of stagflation and unbalanced budgets, economists have not tried to expand the boundaries of their discipline so as to incorporate new material, but rather have simply extended their methodology into new areas to produce even more sophisticated empty boxes.

Modern Theory

Demand

Let us now turn to the theory of demand upon which modern subjective theory has placed so much attention. In focusing on the consumption side of the market, idealized individuals are purported to confront and rank available alternatives based on their willingness and ability to participate in the market. Market participation is ultimately based on individual perceptions and subjective states. The ability to participate (i.e., the person's objective level of income) is of only secondary concern.

Classical economics of the 18th and 19th centuries recognized two parts to value theory: use values and exchange values. The former was rooted in nature and the nature of social men. It was subordinated in economics to exchange value prices since it was exchange value and its transitivity that permitted analysis of the material world and how that world operated in determining supply prices.

Breaking with this conception of use values, individual perceptions of market activity became the starting point for neoclassical thought in the scientific reordering of the 1870s. There remains no better description of the necessary anthropological characteristics attributed to man by this theory than that given by Veblen (1948):

> The hedonistic conception of man is that of a lightning calculator of pleasures and pains; who oscillates like a homogeneous globule of desire of happiness under the stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. He is an isolated, definitive human datum, in stable equilibrium except for the buffets of the impinging forces that displace him in one direction or another. Self-imposed in elemental space, he spins symmetrically about his own spiritual axis until the parallelogram of forces bears down upon him, whereupon he follows the line of the resultant. When the force of the impact is spent, he comes to rest, a self-contained globule of desire as before. (p. 232-233)

Herein resides the reductionist view of human nature which still dominates contemporary economic theory and forms the basis for price determination on the demand side of the market. Jevons (1965) and other pioneers attempted to show that levels of utility or satisfaction derived from the act of commodity consumption and corresponded to given price levels. Using the tools of calculus, the idea of marginal utility was developed (see Majumdar 1966 for an example of attempts to refine and to provide an empirical base for utility theory). By showing that levels of utility declined as the consumption of a particular commodity increased, economists showed that the level of utility of the last or marginal item consumed declined at a rate commensurate with the increase in consumption. Economists thereby linked the external world of the market with individual perceptions.

As the level of marginal utility declined to zero, economic theory pointed out the obvious: commodity prices would decline with increases in consumption since the level of marginal utility itself declined. From this, the idea of biological satiation arose. It pointed out that once the marginal utility of a commodity approached zero, it became a free good with no subjective value to the consuming individual. As satiation occurred for a particular commodity, individuals could choose other, more valued commodities, and thereby retain positive levels of satisfaction and hence positive prices.

There are several things about this model which require comment. The only elements of the external world which require analysis are prices and quantities. The remainder of the social, and for that matter the natural world, is treated as given and invariant. The individual is perceived to be a reactive-agent passively responding to variations in active, external stimuli. The individual is not required to initiate action, he or she is simply a calculator of pleasures and pains. In seeking to gain the former and avoid the latter, the individual is simply required to seek that combination of available commodities which leaves him with the maximum level of satisfaction. It is this search for the highest level of utility with a given budget which defines rational behavior. In asserting that the individual is, in fact, a maximizer, economists are not required to understand the subjective and psychological forces which underlie action. Economic theory simply attributes to all individuals an identical internal process which results in their being able to differentiate between alternatives as to which will yield the highest level of utility.

The rational behavior of any individual is thus described as the attempt to maximize pleasure within an unalterable constraint imposed by the income level of that individual. Rational behavior seen in this light is able to transcend all historical and social boundaries and be seen as natural behavior. It is a common anthropological complex which spans all societies at all levels of social development. Some economists argue that rational behavior so defined is, in fact, not the creation of a particular set of social institutions, but rather resides in a constant human nature (Godelier 1972).

Thus, the problem of choice and preference is structured in such a fashion as to make it appear that the individual is in fact sovereign --free to determine his or her destiny in response to alien external forces--when he/she is not, since the tools that economists have developed may be used to restrain and shape the very "freedom" which allegedly resides within the autonomous individual as we will see below.

I refer here specifically to the concept of "elasticity." The notion of elasticity was developed to measure the effect which changes in market prices or incomes would have on the consumption of a particular commodity. As an empirical tool, the concept of elasticity is designed to measure the degree to which individuals will change their consumption patterns in the face of these changing market stimuli. The resulting ratio of changes in consumption with respect to changes in prices, etc., are labeled either elastic when the ratio exceeds one, or inelastic when the ratio is less than one. The former is labeled a "luxury" and the latter type of commodity is known as a "basic" commodity.

The role of economic measurement, the very essence of its scientific pretensions, is to discern the degree of change in the objective world which can be initiated without drastically affecting the consumption of a particular commodity. *In effect, what economists have done is to provide an empirical measure of commodity perception.* For example, we know that those commodities which are inelastic may in fact have their prices raised by the producer, which will result in higher revenues. The goal, then, of business behavior is to make higher profits by manipulating another's subjective definition of a situation while remaining in the realm of freedom.

A recent article in the *San Francisco Examiner* (Itow 1982) helps to illustrate this point. Researchers at SRI International have developed the Value and Lifestyles Programs (VALS) and which are used by companies to understand influence on consumers. The VAL information is "based on the theory that as people grow they fill certain needs for survival and security and then seek a sense of belonging... Next, they strive for self-esteem and then move upward to 'self-actualization,' developing their inner self and realizing their full potential" (Itow 1982:1).

Interestingly enough, this taxonomy may also be used as a marketing tool by which companies may present their commodities in such a fashion as to appeal to specific groups rather than to the universal market. The information in the VALS study is itself a commodity, costing companies between $4,500 and $45,000.

The direction of the program, though experiencing conflict over whether the means justified the ends, nonetheless concluded that "we are helping people to be more responsive and more responsible" (Itow 1982:2). Such instrumental treatment of another human being for the profit of another is, of course, not so easily solved at the level of moral philosophy about which economists can make no comment as scientists. Given the need for individual market participation the entire perceptual realm may now be profitably exploited even if it means the destruction of the free individual.

In sum, economic theory has arrived at a position where its tools may serve to contradict the proposition upon which economic theory is based. Economists cling to an outmoded but convenient anthropology which suggests that manipulation occurs only when individuals are less than rational. Recent work by Christopher Lasch (1979) contradicts this. He contends that economic man has been supplanted by "the psychological man of our times--the final product of bourgeois individualism" (Lasch 1979:22). If Lasch is correct, psychological man is both manipulated and manipulating; a premise which is far different than the pristine view of economic theory. As we will see below, a similar unsettling situation occurs with the supply-side of the market.

Supply

The theory of supply since Alfred Marshall has revolved around the theory of the firm, the ideal-typical small business. Given the market categories of wages, rents, and profits and their origins in the productive capacities of labor, land, and capital, a theory of production was developed which was consistent with the scarcity notions of neoclassical theory (i.e., the historical process by which the market categories were defined [Polanyi 1957]).

The limits of productive capacity in this context were held to be in line with the idea of diminishing returns. If accepted as a valid proposition, the diminishing returns arguments placed supply limits not in the social world which is external to the analysis, but in the natural world beyond the control of man. Thus, constrained by nature to deal with scarcity, the business community endeavored to place in the market those commodities which could be produced at a sufficiently low price to attract buyers. Attempts to expand levels of output were constrained not by social structures, but by natural forces which forced production prices higher than the competitive market would permit and hence forced businesses to disappear which attempted to violate natural laws.

However, the organization of production is not carried out by specific business units. In the realm of economic theory, highly idealized and autonomous units are at work. With the necessary assumption of competition, the theory of the firm focuses on the representative firm, one "which has had a fairly long life, and fair success, and which has normal access to the economies, external and internal, which belong to that aggregate volume of production... Thus a representative firm is in a sense an average firm" (Marshall 1966:265).

Graphically (see Figure 1), it appears as follows: (1) the long run average and marginal cost curves have their characteristic U shape which is derived ultimately from the assumption of diminishing returns; (2) the marginal cost curve, generated by the cost of the last unit produced, intersects the average cost curve from below at the minimum point of the latter and thus provides an equilibrium point internal to the firm and sets the output level at q; (3) the assumption of competition produces the flat demand curve. In effect, it suggests that a firm may sell an infinitely large quantity of a product at the price given by p. This is the amount that consumers are able and willing to pay for the product, an equilibrium point external to the firm; and (4) where the cost curves and the demand curve intersect, at point a, the internal and external equilibria collapse to set an equilibrium that limits the firm and the individual. For the firm this is where price is equal to marginal cost. For the individual this is where price is equal to maximum levels of utility. This limit is also the point at which the firm earns maximum profits. Hence, expansion or contraction of output away from this point is irrational, since it will only yield losses for the firm. All of these equilibria are produced not by the conscious actions of the entrepreneur or the consumer, but by unconscious and external market forces.

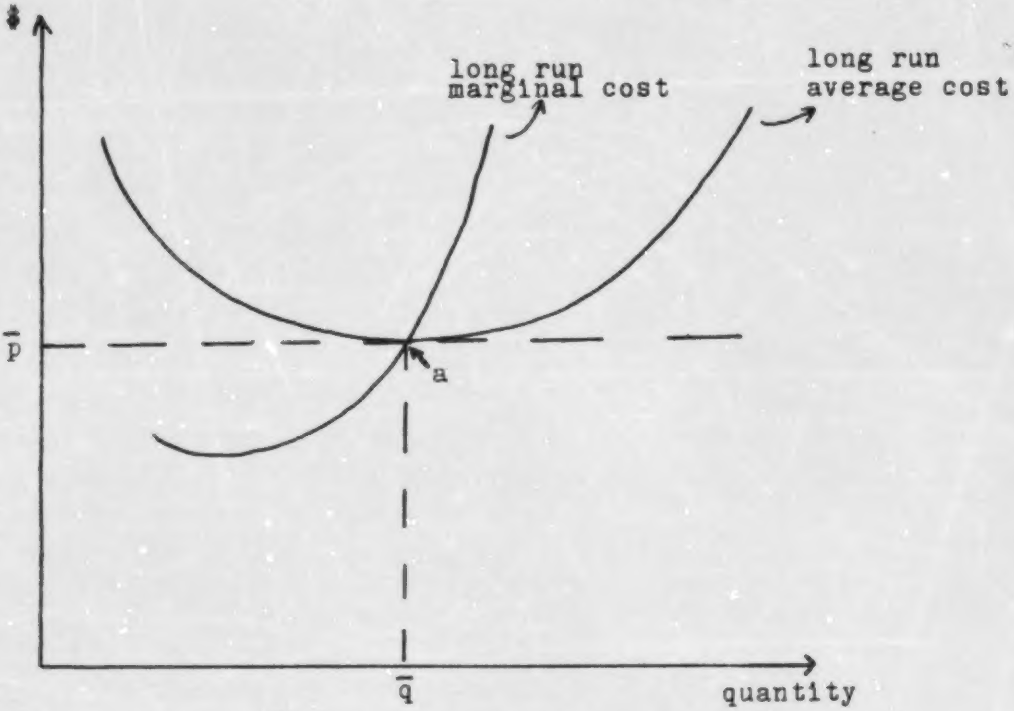

Figure 1

We should recognize that the competitive firm is not an empirical reality but an ideal type, which has attained an almost mythical status. The representative firm exhibits all the characteristics of the reductionist view of the person in demand theory. It is also passive, natural and engages in maximizing behavior, etc. The representative firm is simply a construction illustrating the result of contending forces which produce equilibrium as follows: it is a kind of personality which abstract market processes would produce if left to operate unrestrained.

The resulting picture of small businesses all competing against one another without interacting is theoretically complete and ideologically satisfying. It provides the cultural basis for anti-trust law and the ideological glorification of the entrepreneur. In addition, this set of propositions accommodates the fear of bigness in business as a dominating and un-natural force by showing such extreme size to be "un-natural."

By showing that business acted in a constrained fashion in the market, economists at the turn of the century were able to argue that, in fact, the policy of laissez-faire was a correct one. Interference with the natural workings of the market would distort existing relationships which rewarded all factors of production according to their productive contributions. The development of the marginal productivity theory of distribution showed that in competitive markets all factors were necessarily paid a return according to their contribution to output. Objective forces were thought to determine the wage, rent and profit rates which existed in the economy. Individual efforts were rewarded efficiently, and hence justly, while any interference with these economic forces would reduce the level of aggregate welfare and thus reduce the economic well-being of the society.

Developed in the 1890s, this doctrine did not rupture until the 1920s with the publication of a seminal article by Pierro Sraffa (1926). Sraffa developed an idea that Marshall (1966) had only referred to in an appendix in The Principles; that is, the marginal cost curve had been the crucial element in providing an institutional reflection of the natural limits on the size of the firm. By equating marginal cost and selling price, Marshall had been able to define a competitive equilibrium internal to the firm.

As long as the marginal cost curve was positively sloped, this Marshallian picture held fast. However, when the marginal cost curve and the underlying production functions no longer obeyed the law of diminishing returns, the theoretical situation became dangerous. At the very least, Sraffa posed the economic problem in much the same fashion as had the classical theorists, for he showed the potential for an increasing availability of commodities at constant or declining prices. Moreover, the "natural limits" to commodity availability now disappeared as the cost curve flattened out or became negatively sloped. The objective internal equilibrating forces which limited firm size disappeared. For once, the marginal cost curve failed to exhibit a positive slope. Its limiting role and bridge between the natural and social worlds was broken.

This new conception may be seen in Figure 2.

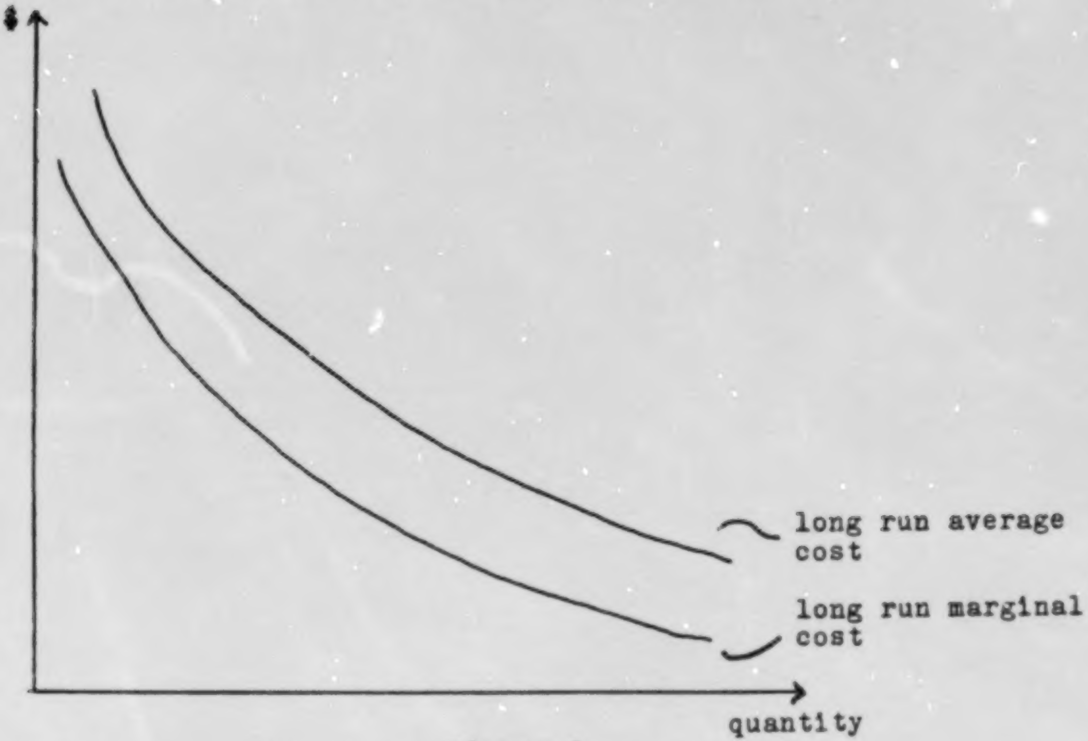

Figure 2

Note first that the cost curves are continually declining. This indicates that the underlying production techniques are not subject to diminishing returns; the natural scarcity assumption is now transparent. If commodities are to be sold at the old profit maximization rates, where price equals marginal cost, then selling prices will decline as output increases. Finally, there is no single point were maximum output can be defined consistent with profit maximization.

In the face of this theoretical destruction, economists were compelled to develop new tools to reestablish a sense of order consistent with the neoclassical paradigm. This occurred in the form of the marginal revenue curve. Derived from the demand curve, but distinct from it, the intersection of marginal revenue and marginal cost was now shown to be the profit maximization point for the firm. That is, the firm could no longer sell an infinite quantity at a given price. It could only expand output by reducing prices along the given demand curve, or alternatively, by moving the demand curve to the right and expanding its share of the market, although this shift would require that consumer perceptions be altered.

In order to rescue the idea of equilibrium and restore order, economists found it once again necessary to retreat to subjective realism. Economic theory now had to show how the elasticity notions used earlier to explain consumer behavior, could now also work to explain production and the actions of individual producers. Thus, regardless of the level at which the newly explained equilibrium occurred, it could now be shown to be the result not of natural forces, but of subjective forces and the changing perception of economic actors.

In providing closure, *the work we have mentioned showed how to extend subjective value theory from the domain of demand to that of supply*. In so doing the concept of equilibrium was restored to conceptual legitimacy. Any distance between the cost curves and the demand curve, which in the Marshallian world would have been viewed as economic rent or monopoly profit, could now be explained as the logical and necessary result of individual preferences to which firms responded. Thus, it was possible to show that both production and consumption were simply responding to external market forces and as such were not actively changing the market to manipulate the market. The passivity of individual units and the overarching unity of the market were restored and extended to the sphere of production.

The other side of this solution had disturbing consequences. It was not longer possible to show that the economy would normally move toward a full employment equilibrium. It was possible that less than full employment was in fact an equilibrium position which could be sustained through long periods of time. The theory of perfect competition as aggregate theory was denied. It is at this point where the work of John Maynard Keynes became important (Keynes 1964).

Depending upon one's political persuasion, he is either famous or infamous for introducing into economic analysis the incidental role of the state as an integral element of economic theory. But it has to be remembered that Keynes' *General Theory* was also predicated on the subjective element, the expectations of the *rentier*. By showing that in the face of surplus, necessary investment activity would not occur, Keynes pointed up the paradox of thrift: while it is possible for an individual to be liquid, it is not possible for a society; it is not possible for a society to simultaneously increase its liquidity, *cet. par*. For this reason among others, Keynes argued that capitalist society needed a regulating force which was external to the market (i.e., the state).

Keynes' work generated the new discipline of macroeconomics. Concerned with market aggregates, macroeconomics has come to be seen as that set of economic ideas which permit the state to control and encourage growth (Gold 1977). This growth, however, is not the growth of the chaotic market but the conscious result of activities by the state to encourage specific forms of capital accumulation in a dual economy. Whether economists are traditional monetarists--like Milton Friedman--or of a "new" persuasion--like Lester Thurow--they all view the state as a necessary force to enhance economic stability.

We are again at a point where manipulation is apparent. While supply and production were initially encapsulated in the theoretically objective unit analyzed by Marshall, the corporation has rarely fit that vision. As a social unit where rates of production and return are often predetermined and not set by objective market forces, corporate activity necessarily is predicated on changing its environment.

Attempts to do so occur within the context of both political and legal action and necessarily require that these forces be included in any analysis of the corporate unit.

The Market

From the foregoing it should be apparent that the idea of the market is somewhat different from the actual institutional framework and set of relations which characterize the production and distribution of commodities. At the level of theory, though, the important thing about the market that still lingers in social consciousness, due in part to the pronouncements of economists, is that the market is a natural, single solution problem-solver. The market is assumed to produce one set of prices and quantities which in the aggregate should equal full employment. The idea of such unique equilibrium has wide popular appeal as folk wisdom but little theoretical guarantee.

What is theoretically interesting, but dangerous as well, is the work that had its base in the 1920s and 1930s. Building off the potential surplus that exists in the market, the idea of a single equilibrium consistent with profit maximization disappeared. What remained was the realization that several different solutions existed, all of which were consistent with the maximization of profits. However, the search for these profits was shown by Keynes to be inimical to aggregate social welfare.

Yet the detrimental effects of the market are still seen as anomalies. The market is, despite all, a sacrosanct institution in which buyers and sellers are still generally viewed by economists as voluntary, autonomous units working to maximize whatever it is they happen to have chosen. All this is still done without reference to external forces.

In addition, the notion of the market has ignored a fact of social life that has dominated economic activity for the last one hundred years. The rise of the corporate world has produced a vertically organized society in which a relatively small number of units dominate a largely rationalized economic scene. The needs of this group are dramatically different from the remainder of the producing units, and these differences do not disguise the fact that these business units, both large and small, are inextricably entwined not only with each other, but with the whole state apparatus (O'Connor 1973).

Yet the predominance of the positivist view of science which economists have unquestionably adopted refuses to admit this. The concern of the classical theorists with the material world has been replaced by a concern with subjective forces which economists do not understand at all.

The "anomalous" situation of unemployment and inflation which David Stockman confronts is not expected. Moreover, the anomaly exists, most importantly, for those who would control or manage the production and distribution of commodities (Gold 1977). Ultimately, however, all economic theory resides in a political Weltanschaung which changes through time (e.g., Duboff 1977).

In sum, the integration of demand and supply components does not yield a natural institution. Rather, the market, both at the single commodity and aggregate levels, embodies created social relations. While economic theory may posit a "normal" situation which defines an equilibrium, that same theory simultaneously identifies the existence of multiple equilibria. The definition of any one of these points as optimal is problematic and political. The attempted realization of it in practice reflects an extra-economic subjective statement.

CONCLUSIONS

The present paper has discussed the myth of "scientific" economics. It was argued that scientific economics is not an objective and disinterested activity. To the contrary, it is a highly social and political activity. The tools of economics are used to control the processes of human development in order to maximize profits and to determine what is presumed to be best for the community. However, the ideological nature and the "manipulative" application of modern economics are antithetical to the presumably natural processes that economic theory alleges to discern as well as to the scientific ideals of objectivity and disinterestedness. By concealing its own internal deviance from the ideals of science, scientific economics is subsequently used to deviate against particular individuals and groups in society.

The development of control techniques in the guise of analyzing market forces reveals the reified character of economics, both as theory and policy, and appears to emerge from the social construction of capital itself. Yet economics does not recognize the existence of a determinant social substrata of economic behavior. Hence, when its theory prescribes mechanisms of social control and the mechanisms fail, it must explain its failure. It must explain observed deviance from its predictions. However, given its presuppositions (i.e., its denial of its social and political nature), it must resort to the individual domain of explanation. It is precisely here where the breakdown of the theory, rather than being seen as such, serves as a pretext for generating scientific and societal deviance. Economics must search out the recalcitrant and perverse individual and/or group and label those who do not conform to its theory as deviant. It must preach restraint of the person or group even as its assumptions and applications continue to be the real source of deviance. Economics, and especially supply-side economics, must blame the victim, exonerate the victimizer, and do so while hiding from the light its own internal deviance and theoretical breakdown. In so doing, economics ceases to explain and becomes a source of chaos and deviance against the society it purports to benefit. Thus, to go beyond the categories of economic theory is to confront the social sources of the deviance of scientific economics. To stay within them, is to reproduce that deviance.

In holding tentatively to "what should be," economists have relied upon a philosophical base which denies their ability to understand and which simultaneously serves as the source of their problem. Moreover, by retaining a focus on the market, they have retained as primary a socially constructed institution whose social origins their theory cannot hope to comprehend, and have retained a view of the world which requires that they decry control in favor of natural market forces while developing tools that enhance control. This constitutes the deviance of economics.

Finally, economists have maintained a voluntarist conception of market participation consistent with their ideological underpinnings and their presumed empiricism. Yet, even modified voluntarist approaches are sadly lacking and overtly contradictory. "It the [conditional form] appears to eliminate the necessity to claim the universal occurrence of particular kinds of conduct in specific kinds of situations... The conditional formulation replaces by postulates what ought to be empirical hypotheses...the assumption of behavior uniformities, as a matter of fact, amounts to the tacit admission that no relevant ones are known" (Burger 1976:175). Such a statement amounts to saying that neither the norms of equilibrium and scarcity which are theoretically imposed nor the deviance of disequilibrium and surplus can be known by the theory which purports to explain them. While the above concepts may be given theoretical content, their empirical measurement is often elusive since, as we have seen, such propositions are "natural and normal" only under very specific *theoretical* conditions.

In conclusion, neither the *disinterested economic scientist nor the effective policy formulator* can eschew the realization that economics may well serve as a form of control theory for the imposition of specific political vision. This tradition has a lineage which is best not ignored; for economics, like other disciplines, operates from a social base (Bernal 1967) which helps to shape the craft of economic theory and which may further account for the increasing realization of the deviance of economics itself.

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NORMATIVE DEVIANCE OF MODERN U.S. SCIENCE:
Its Ideological Legitimation and Philosophical Foundation

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> "Huren, Taenzerinnen und Professoren
> kann ich fuer mein Geld ueberall kaufen."
>
> --Prince of Hannover
> (Boos & Rasmussen 1979:20)

> "My view, based on long and painful observation, is that professors are somewhat worse than other people, and that scientists are somewhat worse than other professors."
>
> --(Hutchins 1963:1)

Although there is no level of scientific development below which it would be impossible to build a beautiful world, science can and should be a powerful tool in its accomplishment. The utopian expectations of the Enlightenment were unachieved, but many are in large part achievable. The obstacle is political: defense of class or self interest; conformity to performance criteria dictated by the existing political-economic system. Thus, there is no inherent reason why the benefits which have accrued to the few should not be available to the many. Science is not intrinsically an agent of oppression and destruction, despite its historical use to provide superior weaponry for purposes of domination and exploitation. Indeed, science's complicity with the military dates back at least to Archimedes. Its role has now become crucial, however, not only in the arms race, but in exacerbating the gap between rich and poor, and enforcing its toleration.

Concern herein is with institutional normative societal deviance. The societal norms of science pertain to the beneficial uses of science while the methodological norms of science (i.e., instrumental norms) pertain to such as the proscription against the falsification of data. Institutional norms of science as an establishment or an "estate" (Price 1965) are distinguished from practitioner norms of scientists as individuals. And norms, images, and behavior can be compared for science and for scientists.

The norms of science comprise part of the ideology of science. They are influenced in turn by ideology in a broader sense, an ideology with a foundation in the philosophy of science, which is itself ideologically infused. The positivist view of science assumes: (a) scientific knowledge is neutral, objective, and value-free; and (b) there is a unitary approach to knowledge (the scientific method, with physics as the exemplar). This constitutes an ideology, "prescriptive scientism," and is the "fundamental false consciousness of our epoch." "Insofar as the practice of the scientific establishment is held to be neutral, while actually justifying the extension of repressive control systems, we can assert that the contemporary self-image of science functions as a technocratic ideology" (Schroyer 1971:297).

The fading positivist, strictly logical-empirical (L-E) orientation of the philosophy of science, which is under challenge, provides a foundation for an ideology which dominates scientific practice. This ideology creates a deluded self-image remarkably similar to both science's stereotypical public image and to a normative ideal expressed by protagonists who exalt science as a noble pursuit of truth and knowledge to overcome the tyranny of ignorance and the ravages of nature. Although the control of science is primarily exercised directly through grants, budgets, policy and priorities, it is enhanced by ideological manipulation which encourages and intimidates scientists to be pliant, passive, obedient collaborators in unquestioned pursuits which are often repressive, destructive, antisocial, and perhaps ultimately catastrophic. Scientists who are ideologically conditioned to be challenging skeptics in their scientific speciality, simultaneously become political eunuchs, doomed to perpetual impotency by the ideology of individualism and professionalism, while their labors are applied to further concentrate power and wealth. An important part of the ideology, essential to its effectiveness, is its self-denial.

In short, *U.S. science is deviant, deviant not only from its prescriptive scientistic image as objective, value-free, and neutral, but more importantly deviant from its normative humanistic ideal*. Recognizing the existence of the ideology underlying the normative deviance of science and escaping its disarming embrace requires understanding its roots and philosophical foundations. It requires an analysis of the political-economic structure which is reflected *in* and justified *by* the ideology, and it requires examination of its manifestations in research programs, data presentation, and interpretation.

Some Examples: Militarism, Poverty and the Misuse of Science

> "Reflective investigators in the year 2072 will draw the following propositions: that the richest and most powerful nation of the late 20th century used the resources of modern science to frustrate the social revolution in a poor and distant land; that, with little protest from men of science, many of the discoveries intended to advance health and agriculture production were turned to the purposes of human misery and crop destruction; that chemical substances, whose long-term effect on human life is unknown, were loosed in staggering and heretofore unprecedented quantities by whites upon Asians; that, the political - and presumably moral - leaders of the powerful nation, when questioned about their actions pursued a policy of lies, half-truths, and studied evasions."
>
> --Jeffrey Race
> (quoted in Pfeiffer 1982:24)

Science and Militarism

While a great deal of attention and concern has been focused on the connection between nuclear physics, nuclear weapons, nuclear power, and the military, the depth of the collaboration of science with the military is inadequately appreciated. As recently as World War I, the response of the War Department to an offer of help from the American Chemical Society was, "Thanks, but we already have a chemist." However, the Manhattan project introduced a new collaborative era.

How were scientists to react to their new status? As Don K. Price (1962) put it:

> To the typical American scientist who still believed that science has helped liberate men from ancient tyrannies, it was disconcerting to be told by a conservative president (Eisenhower) that he had become a member of a new priesthood allied with military power...the intellectual problems involved in this new status are likely to trouble scientists almost as much as the fears of apocalyptic uses to which their discoveries may be put by the politicians. (p. 235)

Moreover, it is distressing to note that while only 2% of the population and 5% of the workers in the productive labor force are employed in the weapons industry "about 10% of industrial workers are (so employed).... For skilled workers this figure is closer to 20%... For the highly qualified scientists and engineers...the figure is between 40 and 50 percent" (Burhop 1978:4-5). And over 60% of federal funding for physics R & D is funded by the military (Gianos & Dittmann 1973).

Yet, it has been two decades since Price voiced his expectation that scientists would be troubled by the possible apocalytpic uses of science. This was before the escalation of the Viet Nam War, before deterrence (albeit with some overkill) was abandoned in favor of first strike capability as the thrust of the U.S. nuclear strategy. Where then are the "troubled scientists" of the "Republic of Science" (Polanyi 1962), serving humanity through the "invisible hand" of Adam Smith, with "scientists freely making their own personal judgement?" Cursory perusal of the control structure of science rapidly disabuses one of the notions of autonomous "impassionate investigators" or "humanist scientists" in a "Republic of Science." Science should be a liberating force, but the success with which science has been recruited into the ranks of the military belies Polanyi's claim that science is neutral, and, therefore, "the aspiration of guiding the progress of science into socially beneficent channels is impossible and non-sensical" (Polanyi 1962:5).

Much has been written about the control of science and the position of scientists in the control structure. Some of this analysis is discussed in a previous paper, "Scientists: Savants or Servants" (Dittmann 1971) and in another entitled "Scholars for Dollars" (Schwartz 1980). Such studies have helped to explain the direction of science and the passivity of scientists, but the problem goes deeper. Essentially, the "purity" of the practitioner's scientific motivation is quite compatible with the practical, applied intentions of the funders and dispensers of grants and the controllers of budgets. The military eliminated support for the "pure" area of high energy physics when it was no longer deemed relevant to military needs. Pure scientific curiosity may have motivated the biologists who studied bird migration in the South Pacific. However, the search for a secure place to conduct biological warfare research with pathogens so dangerous that they could cause a world plague were they to escape was the motivation of the funders using the Smithsonian Institute as a front (Pfeiffer 1969).

Weapon makers themselves complain about the stifling of creative ingenuity in devising ever more effective ways of dealing out death and destruction. Harold Agnew, Director of the UC Los Alamos (Nuclear) Weapons Division, commented, "The basis of advanced technology is innovation, and nothing is more stifling to creativity than seeing one's product not used or ruled out of consideration on flimsy premises involving public world opinion" (quoted in Chomsky 1971:25). Charles Anderson, President of Stanford Research Institute, expressed a similar opinion, "I think SRI people were upset with the notion... that students and faculty should exercise moral control over the work at SRI. This was very upsetting to our professional people" (Anderson 1969:24). The shallow and naive rationalizations for nuclear weapons escalation which sacrificed national security and domestic safety to militarism and imperial ambition are reminiscent of the attitudes of the religiously devout "who thank Providence that they have been saved from the perplexities of religious inquiry by the happiness of birth in the true faith" (Whitehead, quoted by Frank 1957:xix).

This is not to say that concerned scientists should not use their knowledge to develop weapons to assist people in defending themselves from aggressors. On the contrary, it can be argued that there is a humane duty to do so. But determining which party is the aggressor requires political analysis and value judgements, which are deemed "unprofessional" involvements of the proper scientist. Critical examination of the ideological manipulation to which we are all subject is regarded as politics, not science, and therefore beyond the domain of the profession.

The bulk of the scientific community is enlisted in the service of the military, directly or indirectly, and yet this is not the way most university scientists would have it. In a survey taken in Southern California, for example, where the military industry is concentrated, academically-based physicists and chemists indicated that they would devote only 7% of the federal R & D budget to the military, on the average (Gianos & Dittmann 1973). This contrasts with the 53% of the federal R & D budget which the Department of Defense received that year, not to mention the major military R & D components in the budgets of NASA and the AEC. Although not the way that many scientists would have it, resistance is practically nonexistent.

When we take the preceding into consideration, is it any wonder that science has been described as a "Faustian bargain" (Klaw 1971) in which the scientist gains specialized technical knowledge at the expense of humanity, with the scientist uncritically working for the employer or the grantor like a docile proletarian, submitting without complaint to the slave or military mentality ("ours is not to reason why"), and abandoning his humanity (i.e., not taking sides, caring, being committed) in an attempt to adapt to the "Professional ethic" of positivistic scientism--disinterestedness, neutrality, freedom from values and objectivity. Ironically, despite science's lofty claims, no other profession has as high a degree of complicity with the "death and destruction" industry. The motivation for G.H. Hardy's famous toast to pure mathematics: "May it never be useful for anything", is often forgotten. But it is expressed in his reaction to the "war to end war," World War I: "A science is said to be useful if its development tends to accentuate the existing inequalities in the distribution of wealth, or more directly promotes the destruction of human life." (Hardy 1940:121).

Science and Poverty

Even less well recognized than the profound complicity of science with the military is the role of science in creating poverty. Hardy's reference to the utility of science as a concentrator of wealth alludes to this. In part, the imposition of poverty is related to weaponry. The first nations in the world to succeed in applying science to warfare conquered the rest of the world. Their competition for dominion erupted into wars in which they wrought destruction and impoverishment upon each other as well.

Science and technology can also be used to impoverish people without force of arms, even though it may be supported by the threat of military intervention. For example, an end to hunger in the world was heralded with the development of high-yield grains (i.e., the "Green Revolution"). For his contribution, Norman Borlaug, who was backed by Rockefeller funding, received the Nobel Peace Prize (which was also awarded to Kissinger, who played a major role in directing the Viet Nam War, and to Begin, who directed the invasion of Lebanon). But critics foresaw, and studies by development banks have since confirmed, an increase in poverty, especially in South East Asia, from the introduction of the new agricultural technology. Only those corporations or peasants with adequate financing could take advantage of the capital-intensive techniques which required mechanization, irrigation, fertilization, and pesticides. While the greater efficiency of scientific agriculture increased production, it also reduced the prices the poorer bulk of the peasantry received for the same crops that they had previously produced with the same effort and thus resulted in massive impoverishment (Myrdal 1970; 1971).

Industry is even more dependent on science and technology than agriculture, which tends to magnify the effect of a practical monopoly over advanced techniques by the rich, developed nations. Domestically, a similar advantage accrues to larger, more powerful corporations because they have the requisite resources to exploit new techniques when they are developed. Studies by the Max Planck Institute have in fact assessed the role of science and technology in abetting the accumulation of capital, in exacerbating the gap between the rich and the poor nations, and in affecting the international division of labor by allowing industry to disperse highly advanced technological production to sources of low paid, unskilled, uneducated and oppressed labor throughout the world (Fruebel, Heinrichs, Kreye, & Sunkel 1973).

Science, Manipulation, and Control

The techniques of control to which science and technology have contributed range from the relatively subtle to the blatant. Among the former are subliminal psychological methods using motivational research techniques, developed in large part by the advertising industry serving the needs of profit-seeking business, and in which the United States enjoys a tremendous advantage. The latter include economic sabotage (such as bacteriological warfare against agriculture) and overt military intervention. Anthropology, psychology and other social sciences have been enlisted in the search for target populations' superstitions, fears, hopes, and aspirations to be manipulated in order to create public disorder and economic dislocation (as was done in the overthrow of the democratic government in Chile, for example). For further elaboration of these techniques see the writings of: (1) Kathleen Gough Aberle on anthropology (1970); (2) David Horowitz (1969) on recruitment of academic resources in the social sciences; (3) Fred Landis (1982) on the use of subversive manipulation techniques by the CIA in publications in Latin America; and (4) others cited in Dittmann (1971), Arditti, Brennan and Cavrak (1980, Ch. II) and Morgenthau (1965).

Models of Science

An examination of several models that attempt to capture the essential nature of the scientific enterprise may help to clarify some of the reasons for the normatively and humanistically deviant behavior of science outlined above. For instance, some critics emphasize the inherent nature or internal structure of science in seeking explanations for its effects. Jacques Ellul (1964) considers technology (or more broadly "technique") to have an intrinsic compulsion. Science has also been described as having become so well established that it constitutes a new "estate" (Price 1965), or even an "autonomous republic" (Polanyi 1962). De Jouvenel (1963) has theorized that the reins of intellectual influence, having been transferred from the priests to the lawyers, now are in the process of passing to the scientists, who have been described as a "new priesthood" (Lapp 1965), as the "new brahmins" (Klaw 1968), as "the new mandarins" (Chomsky 1967), or as "the new friars" (Barzun 1964). However, the service of the old priesthood to the military and the aristocracy makes one wary of such an analogy for ordination comes not with the doctorate, but by appointment.

Interestingly, despite these grand claims, there is almost a "conspiracy of silence" about the proletarian status of scientists (Gorz 1980), a status which has become more painfully obvious as the availability of tenured positions has diminished, as the days of

Greenberg's (1981) "Dr. Grant Swinger from the Center for the Absorption of Federal Funds" have passed, as the awe in which the keepers of the nuclear genie were held has waned, and as the solitary genius has been replaced by an army of investigators bending to their tasks to further concentrate military and economic power. Yet to U.S. scientists, seeking grants, sitting on "peer review grant boards," serving by appointment (not election), making recommendations (not decisions), where advantage has been taken of their expertise, but where the general criteria for the awarding of grants are dictated within a preexisting budget with preordained priorities and with little influence over scientific policy, the concepts of a "new priesthood" and the "republic of science" must seem droll indeed.

The limited validity of the "autonomous republic" model is enhanced though if it is taken normatively, or if attention is restricted to the scientific elite, analogous to the misleading "star model" stereotype of the entertainment and sports industries. This is the approach Popper (see Easlea 1973) uses in depicting Kuhn's (1962) "normal" scientist: an uncritical thinker, dogmatically trained, who solves "puzzles"; as an "applied," not a "pure" scientist. He facilitates the ascription of responsibility for the egregious misuse of science to the applied scientists and culprit politicians, thus preserving untainted, at least, a core of the purists. Notwithstanding, there are problems even with this attempt to exonerate science by using a definition that puts distance between science and its applications. For instance, it was specifically the elite of the pure scientists who congregated in operation JASON to "brainstorm" for the U.S. military in Viet Nam (Schwartz 1974). Predictably, Operation JASON was portrayed as a purely humanitarian effort to save lives by hastening the defeat of the Vietnamese. There is no intention here to dismiss the intellectual capability of the elite scientific community. I suppose a defense of the élitist model might still be mustered by claiming that the elite were not acting as "pure scientists ought," thereby abandoning the behavioral argument for a fall back normative defense.

In his book *Disturbing the Universe*, Freeman Dyson (1979) provides an apt illustration. He begins bravely enough by describing his work in developing the electronic battlefield as a military weapon in Viet Nam that purportedly provided an alternative to the barbaric "search and destroy" operations and was thought "defensive" (i.e., for the invading U.S. troops!), but he concludes timidly that the electronic battlefield was not morally justified because the regime in South Viet Nam "had no political cohesion and no capable military forces of its own" (Chalk 1982:22). Regardless of the distinctions one may make between Nazi Germany and South Viet Nam, the inadequacy of these two conditions of moral justification become obvious by recognition that they were both fulfilled in Nazi Germany.

Norms and Images of Science

Despite the normative deviance of science, as indicated by the previous examples, two flattering, somewhat varying normative images of the scientist tend to prevail. These two images have contributed to the normative deviance of science. One is that of "The Impassionate Investigator" who:

> with a broad background encompassing many disciplines upon which to draw, seizes upon a problem that attracts his interest (in the autonomous "Republic of Science"). His choice is decided primarily upon the basis of progress in his field of endeavor and upon the inclination and talent of the researcher. He repairs to library, laboratory, or office and proceeds systematically to investigate his chosen topic in an atmosphere of detached objectivity, consequences aside. His results, obtained from carefully tested hypotheses, are then subject to the careful scrutiny of other investigators, many of whom will compare their own investigations and take the new results into account in their own work. So the process proceeds, with iteration, as the unbiased truth emerges, in an "ivory tower," not safe from empirical evidence, but where tenure and economic security enhance objectivity, and the fellowship of other truth seekers sustains an atmosphere conducive to detached scientific pursuits...

Whereas the other image is that of "The Humanist Scientist" who:

> in order to free humanity from the tyranny of the ravages of nature and of the oppression of ignorance provides scientific knowledge which acts as Emancipator and Enlightener. His neutrality enhances his effectiveness in achieving its ultimate, humanistic goals. The researcher's scientific pursuits not only provide an exalted, noble life style for the investigator, but enhance the human condition as science's potent tools subject nature to human dominion and relieve human affliction. (Dittmann 1971:2-3)

The "impassionate investigator" is a personification of Merton's "disinterested scientist" (Merton 1973). However, a practitioner may have a strong, perhaps even a passionate interest in obtaining reliable, accurate results, in discovering, in understanding. Furthermore, indifference to the impact of science on society is not a necessary precondition for valid scientific work. The virtue of knowledge does not justify its nefarious use. The methodological ("instrumental" or "technical") norms of science with which I have no great quarrel, must be distinguished from societal norms.

Self Norms of Science

In a mail survey (Gianos & Dittmann 1974) of samples drawn from academically based physicists and chemists in Southern California, our class in Science and Public Policy found:

1. 72% of scientists (50% of physicists) disagreed that scientists are morally responsible for the technological

spin off of their work;

2. 58% felt that scientists should have 90-100% control over the research budget;

3. 58% felt that scientists should be well informed on world affairs.

Concerning physicists only:

4. 19% felt that scientists should work for the government unconditionally as a patriotic duty; and

5. 31% agreed, 41% disagreed that science required an institutional ethic rather than merely a methodological ethic.

By comparison, Michael Mulkay (1975) found that scientific norms included emotional neutrality, disinterestedness, and impartiality along with such standard traits as rationality, universalism, individualism and organized skepticism. Yet, Mulkay (1975), Merton (1973) and Mitroff (1974), in their sociological approach to science, are struck by the high frequency of deviance from these norms among practicing scientists. This deviance has in fact become so systematic as to constitute "counternorms," each with some methodological justification: "secrecy" often prevails over "communality" as a motivator to heightened effort in order to recognize achievement; ad hominen (reputation) criteria are used instead of "universal knowledge claim" criteria as a more efficient and convenient evaluation technique; and "commitment" is required instead of "emotional neutrality," "disinterestedness," and "impartiality" in order to overcome the usual early disappointments in trying to formulate a new theory or paradigm, which in its formative stages is at a competitive disadvantage with the established theory (which has been subject to a long refinement process), but which may have a strong, essentially aesthetic appeal (also see Blisset 1972; Lipset & Ladd 1971).

However, even when we take the evidence for counternorms into consideration, there is general agreement that science is unlike any other activity, that it requires complete independence, that government should support, but not govern science (Greenberg 1969:29), and that science possesses an internal structure guaranteeing efficiency and fidelity to knowledge which requires no accountability, review or surveillance (Greenberg 1969:338).

The Stereotypic Image of Science

Public perceptions of science and the self-perceptions of scientists conform closely to one another. Mostly, but not entirely, they are restricted to methodological and psychological norms and traits. Texts, popular accounts, and questionnaires surveying adolescents generally provide an image of scientists as humble, objective, neutral, intelligent, patient, open-minded, and dedicated to research, truth, humanity, and country, rather than to money, fame and glory

(Mead & Metraux 1962; Mitroff 1974).

College students similarly characterize scientists as intelligent, *individualistic, socially withdrawn*, self-sufficient, perseverant, *rational, devoted to knowledge*, indifferent to money, *selfless* and *coldly intellectual* (Beardslee & O'Dowd 1973). Also, in the previously mentioned survey of scientists conducted by our class in Science and Society, 91% of the respondents indicated that they believed their work to be socially constructive (Gianos & Dittmann 1973).

Scientists themselves have been actively involved in creating a public image of science. Science has been promoted as an important and practical contributor to material well-being, to U.S. values (e.g., utilitarianism, egalitarianism), to religious values, and as a vehicle for social control. Even as early as the 1870s science began to be promoted as an end in itself (Daniels 1967, 1968) and began to take on tones of the sacrosanct (i.e., "the citadel of the intellect," "the cathedral of knowledge," "the temple of wisdom," "the exalted and noble search for truth").

Utilitarianism reemerged as justification for pure science with the Manhattan project which was so overwhelmingly impressive that, despite mild disclaimers, little effort was required to convince the new patron of science, government, of its expected fruits. As Alan Waterman (1965), retiring president of the AAAS expressed it, "completely free research is highly important in its own right, not solely because of the probability that it will...ultimately produce practical and tangible benefits" (p. 16). Moreover, the "practical and tangible benefits were not to be restricted to the material, economic and military...the scientific method was the ultimate guarantee of the existence of the values of pre-war progressivism-individualism, political and economic democracy, and progress" (Tobey 1971:XIII). In a reincarnation of Desogulier's *Newtonianism: The Best Form of Government*, it was eventually claimed that "American democracy is the political version of the scientific method" (Tobey 1971:13).

In sum, there is general agreement between the scientific community, the public, and prominent advocates of science that both the image and norms of science as a social institution and scientists as individuals are, and should be, basically humanistic, whether the ultimate humanism results from conscious design or from deliberate neglect. Schizophrenically, these strongly value-laden goals are considered to be best achieved by restricting science to objective, neutral, value-free activity. Science is expected not only to provide a foundation for technology leading to material well-being, it is expected to serve as a beneficial religious, moral, and political influence.

The Ideology of Science

The original meaning of the term "ideology," which was coined by Antoine Destatt de Tracy during the French Revolution, was true to its etymological origins: "Science des idees" (Barth 1976), although the Bonapartist reaction developed its own pejorative sense of the term "ideology," for which the adjectives "biased" and "prejudiced" are appropriate. Later, Engels (see Feuer 1959:408) referred to ideology as *false* consciousness. The term "ideology" has since come to refer to a largely unconscious, or unrecognized, or unacknowledged set of values and presuppositions which affects one's judgment, interpretations, conclusions, sense of relevance (e.g., in the choice of measurements to be performed, selection of significant data, form of data presentation), which influences concepts of legitimacy, priority, and propriety, and which enters into the process of theory validation.

The ideology of a society perforce pervades its institutions. Science is not immune. In fact, the normative deviance of science is legitimated and justified by the ideology of science, the relevant components of which are considered below.

Norms as Ideology

As previously discussed, science is expected to be dedicated to humanity and country, devoted to truth and knowledge, productive of practical and tangible benefits (including political and economic democracy and progress), selfless and humble. Schizophrenically, science is also expected to be emotionally neutral and detached, disinterested, objective, instrumentally or technologically rational, loyal to employer, impartial, and coldly intellectual. These latter quite contradictory values are purported to result in the fulfillment of the former values. As with Popper, *the humanistic goals are expected to be accomplished by not caring whether they are fulfilled*.

Physical "Laws" as Ideology

> "Science is more than a 'rhetoric of conclusions,'...it is...a mode of investigation which rests on conceptual innovation, proceeds through uncertainty and failure, and eventuates in knowledge which is contingent, dubitable, and hard to come by."
>
> --Joseph J. Schwab (1962:5)

According to Danto and Morgenbesser, "A law is a true sentence, and a theory is a system of true sentences" (Danto & Morgenbesser 1960:177). Still prevalent in references to the "laws of nature" is the Enlightenment notion used to counter the criticism that science was seeking to bite the apple of knowledge forbidden to mortals. Science was defended as an attempt to achieve greater piety through an understanding of the works of the deity in which he ordained the behavior of nature as part of the creation process. Contemporary conceptual scientific developments are called "Theories" which gain degrees of validity, not ordained "laws." It is also more appropriate and accurate to discuss "scientific theory" rather than "scientific knowledge," a term which still hints at an objectionable and unattainable absolutism. (How could one "know" anything about which there may be only a perhaps overwhelming preponderance of evidence, but not a proof deduced from *a priori* principles?)

Objectivity and Neutrality as Ideology

> "The kernel of the scientific outlook is the refusal to regard our own desires, tastes, and interests as affording a key to the understanding of the world."
>
> --Bertrand Russell

The scientific enterprise is conducted according to methodological techniques which have allowed the development of a consensus understanding of the behavior of nature far beyond anything humanity has been otherwise able to achieve. While scientific methodology does not have the formal structure and codification in its search for understanding of nature that is characteristic of jurisprudence in its search for justice, it has been overwhelmingly successful in its endeavor. In large part it has accomplished the goal of the scientific outlook expressed by Russell in achieving objectivity. As Einstein put it:

> Science as something existing and complete is the most objective thing known to man. But science in the making, science as an end pursued, is as subjective and psychologically conditioned as any other branch of human endeavour--so much so, that the question 'what is the purpose and meaning of science?' receives quite different answers at different times and from different sorts of people.

Recognition of the subjective elements which are always involved in the accumulation and interpretation of data can be traced back at least to Kant. In our thinking we heavily rely on preconceived models, patterns, and terminology, and judgment is always involved in deciding which data to collect and which experiments are more important or more relevant. Prior understanding conditions the selection of relevant variables and the formulation of research programs.

Nevertheless, objectivity and freedom from (pejorative) ideology are worthy goals. Pretended objectivity and feigned lack of ideology lead to a loss of objectivity and reduce our defenses against false consciousness. Yet, objectivity does not imply lack of concern, or neutrality, with which it is often confused. Uncaring calculation and experimentation are not the hallmark of science, they are merely dehumanizing. Moreover, there is not, as Popper would have it, a simple contest between reason and emotion ("He who teaches that not reason but love should rule opens the way to those who rule by hate." Popper 1966:236). Popper's fear of love recalls St. Augustine's fear of and preoccupation with glandular tumescence (Dunham 1964). But passions do not necessarily cloud rationality. Objectivity and rationality can aid us in acting on our cares and concerns.

Much of the misconception is attributable, I believe, to our propensity for dichotomy. A relief from complex grays is provided when choice reduces to two sides of a barricade, here, between the rational and the irrational. Confusion results from our failure to dis-tinguish creativity, experimentation, theory validation, interpretation and consequences in discussing science. Furthermore, the relative importance of empiricism, logic, values, and objectivity tends to vary both behaviorally and normatively.

There is other evidence of the ideology of objectivity in science. For instance, the very passive, third person style of conventional scientific literature expresses the ideology of objectivism. By eliminating the reference to an agent, as well as references to time and modality, an image of objectivity is created, easing its reception in the literature. E. O. Wilson, proclaiming empirical support for the proposition "human beings are absurdly easy to indoctrinate...", converts his claim into a research program: the search for "indoctrination genes" (Wilson 1975; also see Lewontin 1977). While this passive statement conveniently omits the question of whether it is the owners of the mass media who are indoctrinating viewers and readers, *or vice versa*, the grammatical form fits and enhances the ideology of pretended objectivity in science, and increases the acceptability of the statement. Science is reified. Scientists' judgements are portrayed as the passive objectivity of nature.

Values, Theories, and Facts as Ideology

In his "anarchistic theory of knowledge" Feyerabend (1975) suggests "abolishing the distinction between a context of discovery and a context of justification" (Feyerabend 1975:165). From this perspective, the entire historical debate concerning origins and motivations becomes irrelevant to the assessment of the validity of science. Consider vector mechanics: whether Newton was motivated by a desire to overcome the Aristotelian distinction between the Keplerian "laws for peers" in the superlunary sphere and the Galilean "laws for commoners" in the sublunary sphere in order to describe the kind of world god would have ordained if god, like Newton, had been a Whig (Grinnell 1972); whether Newton was motivated by very practical military and industrial considerations as witness his advice to Aston (Hessen 1971); or whether "coffee house hedonism" (Feuer 1963) instead of "protestant puritanism" (Weber 1958) provided the impetus for the rise of empiricism prepared the ground for the Scientific Revolution culminating in Newtonian mechanics and is irrelevant to the validity of Newtonian mechanics.

One can applaud Feyerabend's opposition to "law and order" science in which a strict, formal methodology is imposed upon the pursuit of understanding of the world, but this does not necessarily imply the "anything goes" contrary. Science is a consensus-developing activity with a largely informal, essentially sociological methodology.

The position that theory validation should be value-free and strictly logical-empirical *is itself a value judgement*. Or, as Merton (1973) put it, "The mores of science possess a methodological rationale, but they are binding, not only because they are procedurally efficient, but because they are believed *right and good*" [emphasis added] (p. 55). The "fallacy of levels" should be avoided. A statement about cows is not a cow. The epistemology of science is not science. Logical positivists have difficulty justifying the meaningfulness of methodological statements with their strict empirical criteria except in the indirect pragmatic sense ("procedural efficiency," above), but *theoretical description* is not to be confused with *methodological prescription*.

The debate shifts then to the process of theory validation where the distinction between scientific and other institutions, while not absolutely sharp, is more pronounced. Some groups still attempt to rationalize theory validation, trying to eliminate the human, aesthetic component by (for example) reducing Ockham's razor to races between computers performing calculations with competing theories, arguing that good scientists would not be committed to a theory on aesthetic or other grounds which are not strictly logical-empirical.

Notwithstanding, it has been persuasively argued that in the core of scientific activity, in theory validation, commitment (essentially aesthetic) has been, necessarily is, can be expected to continue to be, and should be, an integral part of scientific activity. Easlea (1973), for example, in his review of the Kuhn-Popper-Lakatos debate, makes such a cogent case that paradigm commitment based essentially on aesthetic criteria has played, does play, should play, and perforce must play in theory validation, that I would reopen debate only under duress. E. A. Burtt (1954) succinctly expressed the historical argument when he wrote: "Contemporary empiricists, had they lived in the sixteenth century, would have been the first to scoff out of court the new (Copernican) Philosophy of the universe" (p. 38).

As the logical distance increases between elementary observations and even more abstract, ever more comprehensive theories, and ultimately paradigms, *greater justification* for identifying the distinction as well as increasing the number of levels of abstraction has resulted. However, the use of the convenient economical term "fact" implies an objectional absolutism, ignoring the linguistic categories and constructions which influence the expression of our perceptions, and which likely influence the perceptions themselves, ignoring the theory-laden character of scientific (and other) terminology.

For economy of expression, observations can be referred to as "facts" for most purposes. Nonetheless, this quite understandable use of more convenient if less precise terminology may allow entry of misconceptions which then propagate and aggravate the problem. The accuracy and reliability one presumes to have achieved in observational "facts" may be parlayed into "proven scientific knowledge" (Lakatos & Musgrave 1970).

Faith in Progress Through Science and Technology: The "Technological Fix"

> "...if we can accelerate the process of scientific revolution everywhere, we shall see our way through the three major menaces before us..."
>
> (quoted in Holton 1965:X)
> --C. P. Snow

> "To stop growth means that underprivileged members of society are denied the chance to improve their lifestyle. To stop growth is to deny the dreams of poor people..."
>
> "...there would be increasing unemployment, a lowered standard of living, a lower level of health care..."
>
> (Offered in defense of nuclear power by N. C. Rasmussen, Chair of the Reactor Safety Study 1977:257).

According to J. B. Bury (1932), the two macro-changes in the dominant intellectual paradigm occurred, firstly, when humanity recognized *itself* as the wonder of the world, and secondly, when the concept of *progress* challenged the idea that humans and society were condemned to pass cyclically through stages with minor fluctuations, but without essential change. Science and Technology provided hope of achieving a better life for mortals on this earth rather than enduring inescapable suffering to reach a better world and a better life at the end of this one.

The depth of faith in the "technological fix" may be difficult to assess, although its manifestations are apparent. Recently Khalilzad & Benard (1980) cited several authors in order to illustrate "overly technology - reliant social expectations" in energy policy. After criticism, Peter Medawar (1973) reiterated his belief that "scientific or technological remedies can be found for most of the hateful and unintended misadventures or miscarriages associated with the advance of technology" (p. 9). One is reminded of the technological fix to the problem of urbanization published in the *Scientific American* in 1899:

> The improvement in city conditions by the general adoption of the motor car can hardly be overestimated. Streets clean, dustless, and odorless, with light rubber-tired vehicles moving swiftly and noiselessly over their smooth expanse, would eliminate a greater part of the nervousness, distraction and strain of modern metropolitan life. (quoted in Dubos 1970:95)

However, in the energy debate, the frequency with which one encounters the argument that concern about the energy crisis is misplaced because science will always manage to pull a "technological rabbit out of a hat" appears to be diminishing, especially with the collapse of the nuclear industry, a favorite "rabbit." Similarly, the attempt to achieve a technological fix to the problem of deteriorating national security has served to aggravate the problem. The unabated escalation of destructive technology recalls H. G. Wells' ominous observation:

> In the record of the rocks, it is always the gigantic individuals who appear at the end of each chapter. (quoted in Dubos 1970:72)

Certainly, we have largely overcome the unrealistically buoyant optimism of the Age of Reason - so much so that much of our literature and expectations have yielded to the "New Pessimism," if not to Dystopias (Sargent 1972), and even Catatopias. Still, uncritical acceptance of new developments in science and technology is often advocated in the name of progress. Nuclear power is a prime example. However, progress is not merely change, nor merely development of technique. "The believer in progress believes in social progress, or else does not believe" (Kallen 1950:10), but a search of a library file catalogue reveals only progress in technique: "progress in brain tumor surgery," "progress in electronic materials," "progress in nuclear reactor theory." Despite the terminology, socio-political-economic change of significance is considered "progressive" only in retrospect.

The concept of progress is infected with "technological rationality," the efficient accomplishment of unspecified or unquestioned ends through technological development. It often manifests itself as local efficiency at the sacrifice of overall efficiency, as Gunnar Myrdal (1970, 1971) indicates has frequently been the case in agricultural policy. The efficient accomplishment of undesirable (or at least unexamined) goals, greater speed in the wrong, or in a random direction, is ultimately not efficient at all. But consideration of goals and consequences requires values and interdisciplinary critical thinking.

Professionalism as Ideology

> "It is the hallmark of the expert professional that he doesn't care where he is going as long as he proceeds competently."
>
> --Herman Kahn (quoted in Lens 1976)

The scientific community is immobilized by a "professional ideology" which condemns social concerns as "unprofessional" and which limits responsibility to a methodological ethic which merely forbids "hashing" the data, plagiarism, and abuse of human subjects. Scientists who concern themselves with responsibility for consequences of scientific activity can be expected to be labeled "unprofessional" and encouraged to "return to science." Academic review committees interpret such activity as evidence of a loss of interest in the discipline and discount the value of ordinarily acceptable technical research.

"Whistle-blowers" (Nader, Petkas & Blackwell 1972) exposing antisocial activities of their military or corporate employer are automatically in violation of the "Guidelines to Professional Employment for Engineers and Scientists," which have been adopted by more than twenty U.S. societies, and which ordains that "The professional employee _must_ be loyal to the _employer's_ objectives and contribute his/her creativity to those goals" (Dittmann 1979:2) (emphasis added). The implication is that the vast and increasing bulk of scientists enlisted in working for the military must be loyal to militaristic goals. In the capitalist sector, allegiance must be committed to the _raison d'etre_ of corporations; the maximization of corporate profit-taking. Encouragingly, a recent AAAS study reported that "codes of ethics are being re-evaluated or drafted...in many societies, and new emphasis is being placed on supporting mechanisms to assist their members who may experience basic difficulties in carrying out the rules of conduct of their profession" (Chalk, Frankel & Chafer 1981:9; also see Callahan 1982:43). However, not all "professional" societies have approved such guidelines. The APS found concern for the conditions of employment _in any form_ to be beyond the purview of their narrowly defined concept of "professionalism." It smacked too much of trade unionism.

Domination as Ideology

The sexist imagery of science, in which the male scientist "probes the skirts of nature, probing her depths, unlocking _her_ secrets, and subjecting her to _man's_ dominion" has a bizarre history (Easlea 1980, 1981, 1982; Merchant 1980; Sampson 1982, and printed herein). It provides vital support for the oppressive uses to which science is applied. This list of ideological components of science is not exhaustive. One could cite the search for simplicity and patterns which obediently appear, the tendency to "geometrize" theory, reductionism, and (at least macroscopic) determinism, among others.

Philosophical Foundations of the Ideology of Science

Much of the ideology of science just reviewed is supported by the neo-positivist logical-empirical philosophy associated with the Anglo-Saxon School of Thought (Radnitzsky 1973). It derives from the logical positivism of the Vienna Circle which, in its original excesses, restricted "meaningfulness" to empirically verifiable science or pure logic (for which, at least in the Edinburgh School [Barnes 1974] the hope of empirical verification still persists). Hence, epistemology, being more than aridly logical and less than empirically verifiable, except in an indirect pragmatic sense, was excluded from consideration. Popper's greatest contribution was in overcoming the resistance to the epistemology of science as a legitimate ("meaningful") area of scholarly endeavor (Popper 1952).

Due to the efforts of Phillip Frank (1957) and others, the criterion for the validity of mathematics was voided of its empirical content and reduced to pure logical consistency. Bacon's "Queen of the Sciences" was no longer a science, or as some philosophers like Bunge (1973) prefer to say, it was recognized as a "formal" instead of a "factual" science--terminology which has the unfortunate consequence of blurring the distinction between logical and empirical criteria for validity. Science seeks and strongly prefers logical consistency, but does not rigorously insist on it, as the Bohr model of the atom attests. Empiricism is the *sine qua non* of science as logical consistency is the *sine qua non* of mathematics. Thus, logic, the descendant of "Reason" from the Enlightenment, became an equal partner in the narrow logical-empirical (LE) criterion for validity.

Certainly there was, and will continue to be, serious concern about the perversions of personal prejudice, parochialism and cultural biases as they distort our perceptions and proclamations about the nature of the world about us. But the justification for an inherently dehumanizing ideology of science was provided by the *Der Wiener Kreis* in its zeal to develop a value-free epistemology and with its desire to help overcome political obscurantism and economic floundering in Central Europe. *Its claims of formal ethical neutrality applied strictly to theory validation, not practitioner motivation (Nagel 1936), but they launched an ideology of science which conveniently ignored this distinction.*

Hence, it was not merely the increasing specialization of modern science, but the neutral, value-free, objective methodological ethic of the logical-empirical philosophy of science which divided political "science" from economics and both from politics, ethics and morality. Philosophic terminology--"physical or natural laws," "observational facts," "scientific knowledge"--represented absorption of elements of the ideology of science, which dialectically depends upon its philosophic foundation.

CONCLUSION

Remedy: A Critical Theory of Science

> "In order to understand not only science, itself, but also the place of science in our civilization, we need a coherent science of concepts and laws within which the natural sciences, as well as philosophy and the humanities have their place." (Phillip Frank 1957) (Of course, as Marx proclaimed, it is not only understanding, but progressive change which is required.)
>
> "...it has come about that a generation so amazingly proficient in the practice of science can be so amazingly impotent in the understanding of it...the state of unselfconscious automation in which science finds itself today is due to the lack throughout its history of a critical school working within the scientific movement itself..."
>
> --Herbert Dingle
> (quoted in Frank 1957:XIV)

What Whitehead (Frank 1957) said about the university may be applied with greater validity to science:

> The idea of science is not so much knowledge as power. Its business is to convert the knowledge of a boy into the power of a man. (Frank 1957:XIX)

A theory must subsume the area which it hopes to explain. We must strive for understanding "adequate to the fullness of the phenomena" (Palmer 1969:100). Even the most specialized field has a history, a *raison d'etre*, an epistemology, a methodology, a socio-political-economic context, a politics, a sociology, a philosophy and, likely, an identifiable ideology. *This effort to explore those philosophical foundations which lead to or justify an ideology which promotes and defends normative deviance in science can be considered as an attempt to contribute to the development of such a critical theory of science.*

Criticism of the "perverse rationality" of the scientific view of science has taken many forms. Weber (1968) distinguished between "formal" and "substantial" rationality. Mannheim (1954) used the terms "functional" and "substantial." Habermas (1973) and Schroyer (1971) cite "instrumental rationality" or "technical rationality." Related ideas are the "one-dimensionality" of Marcuse (1964) and the "compulsion of technique" of Ellul (1964), as is the Pythagorean notion of pure science as a demonstration of class and superiority, justifying knowledge without responsibility (Bernal 1954). *The common thread in these criticisms and theories is the argument that it is not rational to merely promote efficient accomplishment of perhaps irrational ends.*

However, the analytical techniques of positivism have merit despite the tendencies toward reductionism, monism, physicalism, mechanization and the emulation of Euclid and metamathematics as a paragon of logical structure, although they must be balanced by a critical theory of science (Wellmer 1971), of its institutions (Horkheimer 1968), of its role in society, of its impact upon intellectual climate, and of its interaction with political institutions. Clearly, *we need a normative theory of the politics of science*, for which Don Price appealed (Price 1965), despite its current lack of academic respectability.

We need to integrate knowledge and values in order to obtain a vitally necessary sense of direction. Understanding requires context, wholism as well as specialization, an anthropology of knowledge. Empiricism must serve criticism (or to speak Chinese, "Politics [in the general sense] must be in command--not politicians"). Epistemology itself must be subject to criticism. Reason must be joined and enhanced by concern. The "fallacy of objectivity" (Husserl 1965) in data, interpretation, and especially epistemology must be countered. The "quarantine model of science," free of politics and values, must be challenged. Science must be recognized as something more than logic plus semantics. We need a new romantic reaction against the excesses of analysis. Skepticism has long characterized science. Not merely skepticism, but critical thinking is required. Neither humanity, nor any of its works, including the purest of sciences, can be captured within the limits of geometry, time, number and such concepts as matter and principle, as useful as they may be within their sphere of competence. Again, we must strive for understanding "adequate to the fullness of the phenomena" (Palmer 1969:100).

The development of a critical theory of science, the recognition of normative deviance of U.S. science, and a corresponding increase in social consciousness by practitioners will hopefully lead to organized, institutional resistance, or at least to a reluctance to participate in the malevolent misuses of science. This would help, in turn, to marshall progressive humanistic forces in the scientific profession in a movement to restructure our social institutions in a more rational and benevolent form.

The development of a humanely rational science can contribute to the emergence of an enlightened external society. Scientific workers have a duty to be aware of the broad social context of their work and of its social consequences. They have a duty to create truly responsible professional scientific institutions and to participate in efforts to improve the external society. Dignity and respect for one's intellect and for one's conscience and humanity demand no less.

APPENDICES

Manifestations of the Ideology of Science in the Nuclear Power Debate

Examples of ideological influences in the presentation of data portrayed as objective and conclusions portrayed as scientific are drawn from the nuclear industry. It is not certain, however, whether it is all clearly ideological, or merely fraudulent and dishonest.

While it is not expected that the nuclear industry would be able to maintain a detached, scientific attitude toward nuclear power when thousands of millions of dollars in profit and investment are involved, resources have nonetheless been restricted to those individuals and organizations with appropriate scientific credentials. Even with such limitations, an ideology of nuclear power has arisen and is evident in the use of scientific nomenclature and methodologies to justify and to disguise the inherent value judgements and questions that scientists working in this area must confront. Some of the ideological components of the nuclear power debate are indicated by: the terminology employed; the use of transient versus steady state models; the hidden values in cost/benefit analysis to assess risk and safeguards; the use of the Mertonian norms of science to legitimate nuclear power; and prescribed "boundaries" of professional and unprofessional concerns. The examples presented here are offered without any attempt to assess the degree to which they are representative, a task which would require a great deal of additional research.

Terminology

Terminology in the nuclear power industry is one form of expression of ideology and is indicated by attempts to "neutralize" value-laden terms. For instance, the National Reactor Testing Station refers to "power *'excursions'*" (NRTS Report PTR-738) (also referred to as "transients" in WASH-1400) which further reading discloses could be "catastrophic" (*Nuclear Info*. 1979:3). Three Mile Island was classified as an "extra-ordinary nuclear occurrence" over the objection of the NRC staff who argued that this classification required an accident several hundred times more severe (Cohen 1976). The major study of the risk of reactor accidents is called "Reactor Safety Study". (It is restricted as well to Light Water Reactors, not breeders, which would have to be introduced were any really substantial amount of energy to be provided by fission). In Munich, the title of the conference unveiling the German version of the Rasmussen report was changed from "Kernenergie und Riziko" to "Kernenergie und Sicherheit" before the conference convened. An explosion is referred to as "rapid disassembly."

Transient versus Steady State Models

Nuclear power is commonly advocated as a technological fix on energy supply problems and as progress incarnate. Despite such claims we should recognize that nuclear power contributes a minor amount of energy to the economy. Nuclear energy production figures invariably cite *gross* amounts, neglecting the fact that more than half of the energy produced by nuclear power is required to sustain the nuclear fuel cycle. It is, moreover, a very recent phenomenon compared to the duration of its effects due to the extraordinary long half lives of some of the radioisotopes produced. *Since nuclear power has only been in operation a short time compared to the duration of its effects, many of the consequences are accumulative, requiring transient models*. On the other hand, fossil fuels, with which nuclear power is often compared, do have some long term accumulative effects, but they have been in operation long enough that an estimate of health effects based upon an annual rate is not inappropriate as an approximation adequate to the accuracy of the data.

Despite the inappropriateness of the steady state model for nuclear power, almost every calculation of health effects I have encountered in the professional literature has presented data in the form of *annual* rates[1] (e.g., Cohen 1974; Døderlein 1970; Hoyle 1977). When discounting the effects of long-lived isotopes (in this case

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[1] An exception was an IAEA conference on *occupational* hazards, where cumulative effects were presented. Of course, because the number of nuclear workers is a small fraction of the population, the deaths per unit of energy would not seem to be inordinately large. The large turnover of "jumpers," transient workers who accumulate allowed radiation doses in a short period of time and then are terminated, means that a much larger fraction of the population will be exposed to the occupational hazards of the industry.

TH-230 from milling tailings, with a half-life of 80,000 years) Cohen (1976:61) declared it untenable to consider the lives of future generations to be as valuable as our own, but did not offer a discount rate. Perhaps offering a discount rate would make the value-judgements involved in the debate too obvious and would detract from its appearance of scientific objectivity.

Nevertheless, the American Public Health Association Governing Council has adopted a resolution stating, "The *immediate* health costs of nuclear power appear to be as low or lower than that of any other near-term fuel cycle" [emphasis added](APHA 1978:3), and thus basing their estimates on *gross* energy, not *net* energy. In addition, attention is usually restricted to the nuclear power *plant* alone, neglecting the remainder of the fuel cycle in which major hazards occur (e.g., Cohen 1974:35). Comparisons with effects due to energy efficiency or conservation seem to be inconceivable.

In sum, the effect of using an *annual rate* of fatalities as an index to evaluate the hazards of a mode of energy production, instead of *total* fatalities per unit of *net* (not gross) energy produced, while eliminating energy efficiency and conservation as competing alternatives (often neglecting the entire full cycle except the power plant), constitutes an extraordinary value judgement, although the data is presented as objective. The lives of future generations (not to speak of people who would die in subsequent years) is essentially discounted to zero.

Cost-Risk/Benefit Analysis

The cost of safeguards is generally compared to the number of lives saved. The utility of the energy is not questioned. The corollary of this approach is that lives can be sacrificed if the cost of the safeguards is too great (i.e., if the technology is dangerous and expensive [Cohen 1974]).

As has been previously noted in cost/benefit analysis, "There is a profound tendency to consider only readily quantifiable, scientific and technical aspects, to the neglect of social, humanistic facets, and to reduce the social factors to scientific/technical, economic terms which loses the essence of the human content" (Dixen & Lange 1977). When human values are considered, they tend to be quantified, and the people who hold them are depersonalized. The value of a life to the nuclear industry is assessed, for example, as the loss of pay a worker would suffer as a consequence of not being alive to collect a paycheck. Social costs of illness and death have also been quantified (Abt 1975), however, such "external" costs as government subsidies and uninsured damage are systematically excluded in assessing costs, as is standard practice in capitalist economics.

Radiation Standards

One might allow that the establishment of radiation standards would perforce involve significant value judgements, but there are many implicit value judgements beyond the obvious choices between degrees of conservativeness and risk taking. Basic standards are established by deciding the maximum allowable dosage of radiation per whole body or organ. Then an attempt is made to estimate dosage levels resulting from emissions through complex environmental systems. Emission standards are then set so that estimated dosage levels (according to available information) are a conservative fraction of that amount. There is no mention of the amount of energy produced. Consequently, as long as nuclear power provides little energy, as long as the effects are distributed over long periods of time, and are geographically dispersed, it will not be required to meet the same standards as other energy sources in terms of total effects per unit of energy. *Nuclear power is thus treated more as a research venture wherein comparisons are made to natural background or other sources of radiation (such as medical x-rays) rather than a commercial energy source*.

The value judgements as to whether the current generation is justified in generating and using power, leaving a "legacy of radioactive waste,"[2] are made implicitly when estimates of health effects are presented on an annual basis. Nuclear power appears safe when the average annual risk to an individual is considered (the manner in which the data is almost invariably presented). *It appears extraordinarily dangerous when cumulative effects per unit of energy are considered* (Dittmann 1977). However, there are no significant discrepancies in these two different ways of reporting data. The results are compatible with one another, although they signify starkly different values, and provide almost contrary impressions when encountered. The point, of course, is that this is hardly the "objectivity" one is led to expect from scientific data.

Mertonian Norms and Nuclear Ideology

In their article "The Norms of Science and the Public Debate on Nuclear Power," Dixen and Lange (1977) compare the Reactor Safety Study, WASH-1400 (also known as the "Rasmussen Report" even though Rasmussen was only the titular head and not the managing director of the study, and was the only person involved in the study who was not in the direct employ of the Nuclear Regulatory Commission) with the Mertonian norms of science: disinterestedness, universalism, communism and organized skepticism. The report, an in-house AEC project, was portrayed as being an independent scientific evaluation. Much of the data was provided by the industry whose product was being evaluated, as were computer simulation models, and was accepted uncritically with a conspicuous absence of "organized skepticism," and by a research team in institutions which were far from "disinterested." Furthermore, far from being an expression conforming to the norm of "communality," it was initially released without peer review. Ultimately (first by public interest science groups, then by the APS, and finally by even the commissioning agency, the NRC), peer review did make its assessment of "no confidence" in the estimates of *absolute* probabilities, which is the relevant concern.

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[2] A term used by Rasmussen in claiming that it is not immoral to leave such a legacy. ("How Safe Is Nuclear Power?" *Atom 252*, October, 1977:257)

Under the Freedom of Information Act, much has been discovered concerning the motivation for the study. Ostensibly, they were unsuccessful in attempting to show the magnitudes of a potential catastrophe to be tolerable.

CASES IN POINT

A. Harold Lewis Before the American Association of Physics Teachers (AAPT).

Harold Lewis, a professor of physics at the University of California, Santa Barbara, was recently invited by the American Association of Physics Teachers to speak about nuclear power. His arguments, *ex cathedra*, embodied most of the abuses herein criticized, to wit:

1. cost/benefit analysis was couched exactly as just criticized, in a form that implies (without explicitly so stating) that risks are justified if it is *too expensive* to remedy them;
2. implicit discounting of future lives occurred in the citation of *annual* death rates;
3. the cited figures referred only to the power plants, to the neglect of the more severe problems in other parts of the fuel cycle;
4. comparisons were made to other examples (e.g., subsidy to coal due to health costs of black lung disease);
5. the benefit of additional electrical energy (of which nuclear power was presented as the only conceivable source of significance) was unquestioned;
6. energy efficiency, irrational consumption, and conservation were ignored; and
7. nuclear power was described as "cheap," *ex cathedra*, without the hint of an attempt to assess real costs, including "external costs."

There was nothing to distinguish this paper in format, title, or presentation from the other less controversial, less value-laden papers primarily directed to pedagogical methodology. The organizer of the conference, seemingly surprised that there would be any concern about such a unilateral presentation, replied that he had been assured by Bob Eisberg, the chair of the Physics Department, that Lewis would make an "objective" presentation. One wonders whether the ideology of science blinds one to the existence of values when they are implicitly presented in a scientific context, with the further pretense of objectivity.

B. The APS and Nuclear Weapons: The Task Force on Radiation Weapons and the AAPT/APS Course on the Nuclear Arms Race

In 1977, I was solicited by Kosta Tsipis and C. Sharp Cook of the APS to participate in a study of radiation weapons which it was admonished, "will have to be rigorously analytical rather than assertive." When I responded that I would gladly participate if the study was not restricted to "instrumental rationality" which is successful in stopping ineffective weapons like the ABM and the B1, but not in stopping dangerous 1st-strike weapons like MIRVs, MARVs and the Cruise. After this communication, there was no further response.

In a preview of the AAPT/APS course on the Nuclear Arms Race, one of the two organizers presented calculations concerning "electromagnetic pulse" and other nuclear weapons effects. When I suggested that the critical elements in the Nuclear Arms Race were the dynamics of its chronology, the instability of the first strike, and the reduced threshhold of the "countervailing strategy" (P.D. 59) as a counterconventional option, and that detailed calculations were not only rather irrelevant to the main issues but were used as an excuse to avoid confronting the issues, the unshakeable response was that physicists demanded such an approach in order to feel that they were considering physics, not politics or some other realm outside of their appropriate professional concerns, and especially so if values (and other non-scientific considerations) were involved. Indeed, during the course, save for an occasional embarrassed and quickly forgotten comment that considerations as to whether a new weapon was destabilizing (and likely to increase the risk of nuclear war), such reflections were beyond competence, mission and concern.

The above examples were offered as illustrations of ideology in action. Despite its apparent limitations, I believe the organizers of the conference were well intentioned and performed a valuable public service by providing a state-of-the-art overview of nuclear weapons technology as an important technical foundation for the nuclear arms race debate.

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SUMMARY: THREE PERSPECTIVES ON DEVIANCE IN SCIENCE

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When I was initially asked to summarize these papers I was naturally delighted--until I started reading them and realized their complexity, their diversity and their conceptual richness. As a social work educator, I was extremely interested in the ethical issues raised, and although a scientific layman, I also wanted to be fair in reflecting the divergent viewpoints.

It seemed to me that the different papers could be grouped into three categories with respect to their view of the seriousness of the problem. Position One, which minimizes the "seriousness" or "unusualness" of science-deviance, includes Jerry Gaston and A. C. Higgins. Position Two includes Albert Barber, William Broad, Eugene Dong, Brock and Maria Kilbourne, Charles McCarthy, and Paul and Peter Meadows. This position recognizes a substantial problem, but one that should be remediable with certain individual, structural and legal reforms. Position Three, which includes Roger Dittmann, Mike Reed and Edward Sampson, conceptualizes deviance in science as a far more profound situation requiring extensive change within science and also in its relationship to other social institutions.

Having committed myself to this three-part typology based on the seriousness of deviance in science, I began to reflect upon the many dimensions and complexities of this problem. It became clear to me that many other questions needed to be addressed: What is the specific problem; What are the different types of scientific deviance; What are its causes; What are its effects; and, what, if anything, should be done about it? After trying to tease out answers to these questions from each of the three positions, I will finally conclude with some reflections and key policy questions derived from these stimulating and provocative papers.

Position One: A Slight Problem

Gaston sees no serious problem because he is not aware of any empirical data or other reasons to assume that deviance in science is increasing. However, Gaston believes that deviance in science is still a problem, but he argues for extreme caution in drawing conclusions about the amount of such deviance: "We can have no confidence in the conclusion that such behavior is increasing or that the behavior which is discovered is sufficient evidence to allege that it is only the tip of the iceberg." He even questions whether recent reports of deviance in science will really erode public confidence in and support for science. He recommends more emphasis on replication of research, the "objective, public, and swift handling of deviant cases" and increased vigilance by the scientific community so that "the most negative consequences for the future of science do not become a self-fulfilling prophecy."

Higgins' position would seem to be in general agreement, although for a different reason. Higgins' historical analysis of deviance in science reveals something interesting, indeed. Deviance in science has always, it seems, been an aspect of the scientific community, and there is no great need for alarm. He is more concerned with the games played by the science watchers--games which make or break scientists in the process of image management for science. This "pretense of Great Science...becomes the greatest barrier to understanding what really goes on in science.... The publication of recent science scandals may be self-serving.... By focusing on specific areas of research in which frauds occurred, other areas of science are left virtually unexamined and untouched." Thus, the taxpayers may feel reassured that science is truly self-policing, while much more serious theft of public funds is quietly ignored. "With these few scapegoats, Defense Department R and D looks positively clean...And University administrators can persist in whatever passes for control of research so long as they can wring their hands over, say, Summerlin's mice." Whether covering-up for some scientists, or pretending indignation about the cheating of others, these "sages of science are ultimately loyalists who see the game of science as played according to the rules of a fraternity: there is no whistleblowing." With this last concern of Higgins, we are nudged toward a more concerned perspective of deviant science within a somewhat broader social context.

Position Two: A Moderate Problem

Our second group is composed of Barber, Broad, Dong, the Kilbournes, McCarthy, and the Meadows--all of whom are more concerned with the serious implications of certain kinds of deviance in science more than the first group, but who also are confident that the situation is manageable. Broad challenges the generally sanguine views on deviance in science when he says, "despite the widespread faith among scientists in the self-correcting nature of science, there is evidence that fraud often is not automatically cast out of the corpus of science, and that, in fact, the phenomenon of fraud may be on the rise." Rather than ignoring these allegations , the Meadows suggest that they are worth studying because they "can tell us some valuable things about contemporary science which we really need to know and understand."

Dong points to some of the implications of misrepresentation in science. These include: the introduction of false information into the scientific literature, wasted resources, improper personal benefit, the creation of negative peer models, and the loss of public confidence, etc. Barber elaborates: "Falsification of data is of major concern because such activity is incompatible with the basic processes on which science depends.... It also weakens the principles on which the self-regulation of science by scientists depends." The Kilbournes further observe that due to the central role of science in all of our lives, "the costs of deviance in science may be greatest where science interfaces with other institutions and/or societies."

If we accept the position that science-deviance is a serious problem, exactly what is it specifically? Several authors say that it is a normative anomaly and that policies have failed to preserve standards. For instance, McCarthy notes: "because some allegations of misconduct have proved to be true and because the associated publicity has been widespread, the integrity of the entire research community has been placed in a shadow." Dong, however, focuses on NIH policies and believes that, "Past NIH policy has failed to protect the Public's interest in dispersing funds fairly and wisely, and in assuring that alleged scientific observations were in fact made." Why is this bad situation likely to persist? Dong claims that NIH fails to recognize the criminal nature of submitting false data, fails to take into account institutional interests in minimizing exposure of wrongdoing, fails to obtain basic physical evidence (i.e., laboratory notebooks, interview complaints, etc.), fails to protect the identity of "whistleblowers" from Institutional Administrators, and most importantly, fails to utilize both scientific and forensic experts to evaluate the primary documents in question.

Moving beyond the sad situation at NIH, the Meadows furnish us with a succinct roster of deviance in science: forging, trimming, cooking, refusal to replicate, the concoction of false data, cover-up, plagiarism, punitive administrative acts, collective mea culpas, and the approval of grant continuations when they should be terminated because of deviance. They set up a typology of research fraud which includes ethical, technical and economic categories. In an earlier paper, Kilbourne, Kilbourne and Higgins (1981, and printed herein) distinguished the white-collar crimes of scientists (e.g., fraud, plagiarism, theft, misappropriation of funds, restrictions of competition, sabotage, and the violation of the protection of human subjects) from the occupational deviations of scientists--departures from those norms specific to science (e.g., ad hominem attacks, dogmatism, secrecy, shoddy work, milking data, etc.). Barber adds to this list: the abuse of confidentiality, inappropriate multiple authorship, premature and delayed reporting of data (also see Gaston), and the falsification of bibliographies.

The causes of deviance in science are complex as noted by members of Position Two, but let us review a few of them. Broad believes the changing environment of scientific research tends to encourage fraud: the growth of large elites at major universities; the cluttering of the system of scientific communication with meaningless papers; the careerist nature of contemporary science; and the "race for priority, grant renewals, tenure, and quick success." Kilbourne, Kilbourne, and Higgins, who believe that the structure of science tends to reflect and to reproduce the structure of society, see deviance in science as occurring for many of the same reasons that deviance occurs in other professional and occupational groups in society: excessive competition, differential opportunities, careerism, rising expectations, value conflicts, the failure of myths, vested interests, etc. Gaston, too, suggests scientific deviance must be attributed to social conditions, and although not quite certain of what they are, suggests a possible relation to anomie theory, along some of the lines earlier developed by Kilbourne, Kilbourne, and Higgins. The Meadows ask us to appreciate the situated nature of scientific deviance, and direct us toward understanding how what goes on both inside and outside of science, at both the institutional and personal levels, affects the occurrence of deviance in science. They want us to take special note of the "industrialization of science" in this regard. Dong even suggests that the policy of NIH may contribute to the problem by indirectly providing the malefactors with a bureaucratic and administrative cloak by which to hide their misdeeds. Another example of teamwork in science deviance is given by the Kilbournes, "Deviance in science and deviance against science are closely related to one another.... We suspect a kind of bi-directionality of influence between these two types of deviance."

The solutions suggested by the Position Two authors include a variety of changes ranging from focusing on the behavior of individuals to that of institutions and even the federal government. In a general observation, Barber makes the point that "research integrity will be assured only when recognition of its importance is appreciated and the responsibility for its protection is fully shared by all who do research." The simplest and most straightforward recommendations suggest closer observance of the normative standards by scientists themselves, such as efficient and fair handling of suspected cases of fraud. Naturally, several of our scientist-authors suggest more research on deviance. And they also suggest that more attention be paid to "the socialization of recruits to science or participants in science organizations" (The Meadows). The same theme is echoed by the Kilbournes, who suggest the need for a greater emphasis on training in ethics at the graduate level.

As noted earlier, Higgins mentions the anti-whistleblowing tendency within science, and Dong notes the typical failure to protect complainants. The Meadows suggest that there must be concern "for the safety and security of these whistleblowers who are often the innocent victims of somebody else's fraud." The Kilbournes recommend the elimination of the informal legal exemptions presently enjoyed by scientists and the enforcement of those laws regulating white-collar crime, but they also see a simultaneous need for explicit procedural safeguards within science and a long-term national commitment to science to counter pressures toward deviance. Dong's major recommendation is the more vigorous application of Title 18 of the United States Code, Section 1001:

> Whoever, in any matter within the jurisdiction of any department or agency of the United States knowingly and willfully falsifies, conceals or covers up by any trick, scheme, or device a material fact, or makes any false, fictitious or fraudulent statements or representations, or makes or uses any false writing or document knowing the same to contain any false, fictitious or fraudulent statement or entry, shall be fined not more than $10,000 or imprisoned not more than five years, or both.

Dong believes that "NIH can do no less to meet its legal and fiduciary duties as a public institution."

Instead of looking toward changes in government oversight, Barber stresses changes in the ways academic institutions handle allegations of fraud. He offers a list of very specific and impressive ways to detect, investigate and prevent fraud. He believes, "It is the special responsibility of principal investigators who serve as mentors within the laboratory... The responsibility cannot be readily assumed by administrators, journal editors or granting agencies... They are too far removed."

The implication of these views, I think, lead us toward the consideration of a more complex and urgent relationship between science-deviance and society. Thus, we move on to consider the Third Position.

Position Three: A Serious Problem

Dittmann, Reed and Sampson all seem to agree on the extreme seriousness of deviance in science, although they are concerned with different aspects. The different aspects, however, all deal with the deviance of science. Dittmann focuses upon the inextricable relationship between U. S. science and militarism, impoverization and other egregious misuses of scientific domination. Reed focuses upon the discipline of economics and how it has historically evolved to be used to legitimate, control and for expressed "political vision," rather than to facilitate our understanding of the economic forces shaping our lives. Sampson, too, attends to the deviance of science, but in relation to such legitimating social theories of classism, sexism and racism. All three authors criticize the science-wide assumptions and methodologies which justify such practices, and try to herald a problem of much greater concern than the concern about either deviance within or against science.

Dittmann claims that, "U.S. science is deviant, deviant not only from its prescriptive scientistic image as objective, value-free, and neutral, but more importantly deviant from its normative humanistic ideal...it is enhanced by ideological manipulation which encourages and intimidates scientists to be pliant, passive, obedient collaborators in unquestioned pursuits which are often repressive, destructive, antisocial, and perhaps ultimately catastrophic." An ideology of science has developed to legitimate the institutional normative deviance of science. Its ideological components consist of: norms, physical laws, objectivity/neutrality, facts, theories and values, the faith in progress through science, faith in the technological fix, professionalism and domination. And it rests upon a philosophical foundation of logical-empiricism.

Reed concerns himself with the "disjunctive relation between the real and ideal worlds of economists" and how this state of affairs reflects inherent values in the discipline of economics as well as how those values have shaped purported scientific fact. Modern economists have created out of subjective value theory an ideologically bounded view of the world, as indicated in their use of such concepts as supply, demand and market to convince people that there are natural and universal market forces "out there" when there are really not. The deviance of economics is evident in its pretensions of being a "disinterested" science when, in fact, it is used to manipulate and control. More important than what Reed says about the failure of modern economics to predict and explain economic behavior, however, is what he has to say about how it explains its failure:

> Economics must search out the recalcitrant and perverse individual and/or group and label those who do not conform to its theory as deviant. It must preach restraint of the person or group even as its assumptions and applications continue to be the real source of deviance. Economics, and especially supply-side economics, must *blame the victim, exonerate the victimizer*, and do so while hiding from the light its own internal deviance and theoretical breakdown. In so doing, economics ceases to explain and becomes a source of chaos and deviance against the society it purports to benefit.

Sampson recognizes the deviance *within* science--"the pursuit of winning at any cost"--but then he steps outside of science to view the institution from a broader epistemological perspective. According to Sampson, "Deviance that is *of* science represents a fundamental distortion in values that occurs when only one framework becomes valid for defining knowledge and so cannot be applied critically to uncover the values that undergird and support that framework itself." Thus, the deviant quirk would seem to be the systematic denigration or denial of other sources and types of knowledge that fulfill important functions for mankind. When scientists sweep these other kinds of knowledge aside as being irrelevant to the search for truth, they destroy part of themselves. The truth-seeking tool slips back and cuts the hand of the user. We see a transformation, a "fundamental inversion of this instrument (the mind) from being a tool of mastery to its own unruly Other; the remedy has become the source of the poison."

From this philosophical diagnosis of the problem, what do the authors of *Position Three* recommend? In a general sense, all recommend a "critical theory" of science and lay claim to the ways in which positivism has been employed to disguise the ideological imperatives of their respective scientific domains. Reed says, "To go beyond the categories of economic theory is to confront the social sources of the deviance of scientific economics.... To stay within them, is to reproduce that deviance." Dittmann would go a step further and suggest that *all* science must give up its "scientistic" ideology so that scientists can play a critical role in the kinds of knowledge they seek and the way knowledge is used. "The development of a critical theory of science, the recognition of normative deviance of U.S. science and a corresponding increase in social consciousness by practitioners, will hopefully lead to organized, institutional resistance, or at least to a reluctance to participate in the malevolent misuses of science... Scientific workers have a duty to be aware of the broad social context of their work, of its social consequences.... Dignity and respect for one's intellect and for one's conscience and humanity demand no less."

Sampson seems to go even further in calling for a fundamental shift in world-view, a "deconstruction of mastery", a move away from a patriarchical mode toward a "gentler attitude toward nature." There would be less emphasis on *mastery* of nature and more on participation and receptivity. "A new metaphor...is necessary lest we literally employ one view of progress to end all progress. I am not simply referring here to the nuclear arms race, but to all forms of destructive domination and exploitation...between men and women, between whites and people of color, between those who presently possess economic dominance and those who remain part of the world's underclass."

CONCLUSION

The authors of our Three Perspectives on science deviance illuminate different floors in the grand old mansion of science-in-society. Position One offers a low-key examination of some of the dark corners in the oldest basement section followed by some up-to-the-minute and sensible policy recommendations. Position Two turns up the voltage of inquiry, generates more wattage, more light, and more heat. Many more hidden aspects of deviance are exposed to light, with a corresponding increase in the number of recommended changes. Position Three takes us to the top floor where the light of inquiry is somehow transformed to a laser beam which goes right through the conventional boundaries of science and probes its connections with other social institutions of our wider society.

For me, the most interesting contribution of Position Three is not its concern with bad science--sloppy, dishonest, and fraudulent--but, rather its penetrating questions about good science, research informed by the highest academic standards of Western thought. Perhaps, the fundamental question posed by the Third Position is: Can certain aspects of scientific inquiry be in fundamental conflict with other important values of our pluralistic world? To say that this is an old question for science is not to denigrate its importance.

Philosopher I. Berlin (1969) raised a similar question about the assumed compatibility of all positive values, presumably including science:

> One belief, more than any others, is responsible for the slaughter of individuals on the altar of the great historical ideas...(namely) the conviction that all positive values in which men have believed must, in the end, be compatible and even entail one another. (p. 167)

History is filled with examples of good science clashing with other good social values of the day. For a long time this conflict was viewed as science against superstition or religion, and we knew that eventually science would be vindicated as the best long-term friend of the human species. Those were such optimistic years with our firmly held faith in science-progress, which would continually shape the world into a better and better place for humankind. Then, just when the science-versus-religion controversy seemed to be settled in science's favor, the nuclear age was born and the science-versus-humanity debate began. Perhaps, this is one reason that we are talking publicly about deviance in science today, for there is some strong underlying public concern. It would be so much easier to deal with deviance in science if if were simply bad science stemming from dishonest individual scientists. But instead, the issue seems to stem at least partially from the bifurcation of philosophy into science versus ethics, truth versus goodness. Now that we have cut them into a kind of mind-body dualism, the question is can we put them back together in time to save the patient, meaning humankind.

Viewing Heisenberg's principle of indeterminancy between the speed and mass of an electron as an analogy, perhaps there is a similar principle of indeterminancy between the truth and moral claims of science. Then, the more doctrinaire and the more exclusive the truth-claims of science, the more vague become its pretensions to moral authority. The science that claims a monopoly on the road to knowledge, forfeits its claim that it is a good road, or that the destination will be constructive for humankind.

Robert L. Sinsheimer (1978) approaches this ethical issue by a different route--not by questioning the exclusive truth-claims of science--but by puzzling over the rate and type of specialized knowledge generated by science:

> Can there be 'forbidden'--or, as I prefer, 'inopportune' knowledge? Could there be knowledge the possession of which, at any given time and stage of social development, would be inimical to human welfare--and even fatal to the further accumulation of knowledge. Could it be that just as the information latent in the genome of a developing organism must be revealed in an orderly pattern, else disaster ensue, so must our knowledge of the universe be acquired in a measured order, else disaster ensue. (p. 23)

If Sinsheimer's question is on target, who is responsible for this ordered development of scientific knowledge? When J. Robert Oppenheimer tried to discourage the further development of the hydrogen bomb, his loyalty was challenged and he was destroyed as a scientist. Unfortunately, he did not live long enough to see his vindication by history.

This symposium has generated some thoughtful and interesting questions, but I fear their answers must await future AAAS symposia. As a social worker, however, I am deeply pleased that these issues are now *out of the closet* and considered a proper subject for open public discussion between scientists and nonscientists. The science that cannot tolerate such scrutiny, that takes itself too seriously to acknowledge self-imperfections, makes *bad* religion.

A psychiatrist friend of mine, Margaret Saino (1964), once observed: "The role of religion is to tighten people up. My role is to loosen them up." A "loosened up" view of science might also not be such a bad idea. As Pascal observed; "There are two equally dangerous extremes, to shut reason out, and to let nothing else in" (quoted in Eisley 1978:190). This collection of symposium papers on science-deviance indicates that we are avoiding these two extremes and intend to continue asking more tough questions about the role of science--

both deviant and conforming--in relation to the long-term welfare of humankind.

Let me conclude by identifying three key policy questions involving science deviance, primarily because all of our contributors agree on the need for *some* type of policy reform. The fact that many different types of changes are recommended reminds me of the fabled blind men describing their tactile experiences of separate parts of the elephant's anatomy. In similar fashion, we have heard divergent reports on deviance against science, deviance in science, and deviance of science. In briefly reviewing these three concepts of deviance, let us see if we can identify an appropriate sensitizing question to guide further efforts at policy reform.

Deviance against science, ways that science is "sinned against," includes exaggerations of the amount and/or the seriousness of actual deviance, embellishments and games of the science watchers, and both covert and overt attempts to suppress free, rational, and critical inquiry. The key policy question is: *How do we protect the institution of science from forces which threaten to weaken or destroy its effectiveness*?

With respect to deviance in science, we have heard about problems with the instrumental norms of science and forms of deviation ranging from minor to serious infractions such as ignoring anomalies when devising a theory, self-deception, deliberate falsehood, and outright fraud. These various acts can also be conceptualized on a continuum ranging from occupational deviation to white-collar crime. But whatever way this deviation from internal norms is viewed, we find our contributors offering a diverse list of policy reform suggestions, including such things as more critical attitudes in the teaching and studying of science, better training in science ethics, revision of the instrumental and quantitative norms of scientific and research organizations and revised supervision and control by grant awarding agencies such as NIH and HHS. The key policy question for this area is: *How can we protect the public interest, the effective internal functioning of science, and also provide due process and fair treatment for alleged deviants*?

Moving beyond concern with the internal instrumental norms of science, we now consider the broader societal norms and the deviance of science; the role science is supposed to play in society and the effects it is expected to have on the world. Concern for societal values requires a broader set of postulates--not starting with the assumption that science is only a set of logical constructs--but rather recognizing William Broad's "dark side of science"; that is, science as an activity which involves the whole panoply of human motivations including such negative ones as passion, prejudice, and propaganda. This is even more important when we recognize the vested interests in society which may be served by what Roger Dittmann refers to as a particular Ideology of Science. Without recognizing this other side of science, how can we explain why American nuclear scientists created the Hiroshima option, or how German and Japanese scientists were willing to do their murderous and insane experiments on human subjects during World War II? Or what will stop our best scientific minds, in the name of either "pure science" or national defense, from creating the final apocalypse of the world through some new form of biological warfare?

Besides the breakdown of societal norms in controlling the activities of scientists, there is the related fragility of the instrumental norms of science when their claim to truth becomes a bit excessive. For example, the rational empirical method as described by Bacon is considered the final judge and jury of knowledge, "...when only one framework becomes valid for defining knowledge and so cannot be applied critically to uncover the values that undergird and support the framework itself" (Sampson). Thus, the policy question for this area involving the deviance of science is: *How can we encourage the balanced use of a variety of aesthetic, moral, and intellectual standards along with instrumental reason so that science can truly serve humanity as well as the other occupants of our planet earth*?

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